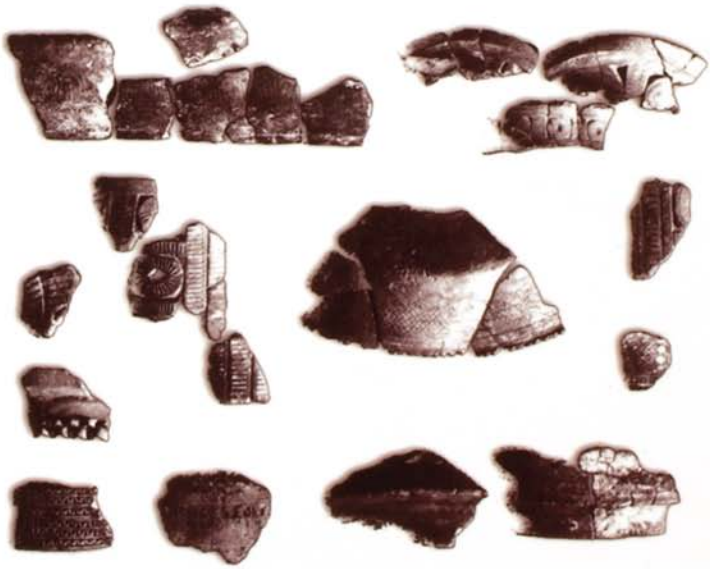
Archaeology and Language II

Archaeological Data and Linguistic Hypotheses



Edited by Roger Blench and Matthew Spriggs

ONE WORLD ARCHAEOLOGY

ROUTLEDGE

29

ARCHAEOLOGY AND LANGUAGE II

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ARCHAEOLOGY AND LANGUAGE II

Correlating archaeological and linguistic hypotheses

Edited by

Roger Blench and Matthew Spriggs

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Cum remotae gentium origines historiam transcendant, linguae nobis praestant veterum monumentorum vicem.

Gottfried Wilhelm Leibniz, *De originibus gentium*

There is no tracing the connection of ancient nations but by language; and therefore I am always sorry when any language is lost, because languages are the pedigree of nations. If you find the same language in distant countries, you may be sure that the inhabitants of each have been the same people; that is to say, if you find the languages are a good deal the same; for a word here and there the same will not do.

Samuel Johnson, quoted in James Boswell,
The Journal of a Tour to the Hebrides, 1785

If we possessed a perfect pedigree of mankind, a genealogical arrangement of the races of man would afford the best classification of the various languages now spoken throughout the world; and if all the extinct languages, and all intermediate and slowly changing dialects had to be included, such an arrangement would, I think, be the only possible one . . . this would be strictly natural, as it would connect together all languages extinct and modern, by the closest affinities, and would give the filiation and origin of each tongue.

Charles Darwin,
On the Origin of Species, 1859: 405

To seek, by the multiple routes of anatomy, physiology, history, archaeology, linguistics and even palaeontology, what have been in historic times and in the ages which preceded the most ancient remains of humanity, the origins, the affiliations, the migrations, the mixtures of the numerous and diverse groups which make up the human species.

Paul Broca,
'La linguistique et l'anthropologie', 1862: 264

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Preface

The relation between the present volumes and the Third World Archaeological Congress (WAC), held in New Delhi in December, 1994, is complex. Events at the Congress have been described in some detail (e.g. Bernbeck and Pollock 1995; Colley 1995; Golson 1995; Hassan 1995) and need not be further touched upon. Some of the chapters were presented as papers in the Congress, as part of a five-day session containing some eighty papers on Language and Archaeology, while others were commissioned for these volumes. In some cases, scholars who presented papers at the Conference have substantially revised their work or even divided it into several chapters. The object has been to develop as comprehensive a coverage as is practical of the issues raised in this area, both geographically and methodologically. These books should therefore not be regarded as proceedings, but as ideas stimulated following that meeting.

Issues of nomenclature, style of data presentation and editorial principles are dealt with below. The introduction is divided into two parts: a general introduction, dealing with the broad issues raised by the interface of archaeology and language, and an introduction specific to this volume.

TERMINOLOGY AND METHOD: SOME EDITORIAL PRINCIPLES

Terminology

An issue thrown into sharp relief by pulling together chapters that in principle undertake the same enterprise in very different intellectual traditions is the wide variety of terminology used to describe the same phenomena. This is nowhere more apparent than in the case of language sub-grouping. The terms phylum, stock, family, branch, section, group, sub-group, language, lect, communalect and dialect are thrown freely around without any clear definition that could assist someone in another region to apply them consistently. This is not to say that the literature is not well endowed with attempts

Table 1 Definitions of language groupings

Term	*Percentage range*
Phylum	5–12
Stock	13–28
Family	29–45
Subfamily	46–78
Language	79–81
Dialect	above 81

Source: Wurm and McElhanon (1975: 152)

to define these categories. The most common of these are in terms of lexico-statistics. Lexicostatistics provides mathematical definitions of the relations between one language and another and therefore would seem very suitable for concrete definitions. For example, one well-known use of this system was applied to the languages of Papua New Guinea (Table 1).

The use of a table such as Table 1 depends heavily on the faith of individual linguists in lexicostatistics. If it is possible for languages to be 'mixed' – i.e. to draw a significant proportion of basic vocabulary from two or more unrelated languages – then lexicostatistics will give contradictory results. It used to be denied that mixed languages existed; then, when this view became untenable, that they were very rare. Mbugu (Ma'a) in Tanzania appears frequently in the literature exemplifying this sort of rarity (Mous 1994). However, Oceania has supplied some of the most striking examples of 'mixed' languages, such as Maisin or Magori (Dutton 1976; Ross 1984), which create problems in applying the lexicostatistic method. Since the work of Thomason and Kaufmann (1988) it is increasingly accepted that this type of language-mixing may in fact be quite common. The effect of a synchronic perspective on language description is that extraneous elements in the lexicon have been assimilated and are no longer evident. If we identify a mixed language in the present it is because we can still identify its components. Assuming that these types of language mixture occurred in the past (and probably did with greater frequency) it may well be that many present-day languages are 'mixed' but that their elements are no longer so easily discerned.

As more syntheses of world languages appear (notably, Ruhlen 1991) a consensus on terminology is slowly emerging. The most important of these is the use of 'phylum', now applied to the large well-known and reasonably established families of languages such as Indo-European or Uralic, but more controversially extended to any language grouping whose external affiliations are not well established or remain highly controversial. This can mean that an individual language may represent a phylum; thus Japanese/Ryukyuan is generally considered an isolate and is usually referred to as 'Japonic'. Indeed north-east Asia represents an intriguing cluster of either very small language groups or individual isolates; these are generally considered to be phyla (see Janhunen, Ch. 8, this volume).

The term 'stock' has remained in discussions of Pacific, especially Papuan, languages but has not been widely adopted outside; most linguists probably use 'family' as the next level of relationship below phylum. Indeed, Indo-European scholars, the most conservative sub-group of historical linguists, remain unused to referring to Indo-European as a phylum. Between stock and language something of a free-for-all obtains; branch, section, group, sub-group are used quite freely, and no fiat from individual scholars is likely to change this situation. 'Language' is generally considered to be a group of speech-forms whose speakers can all understand one another without considerable effort. Below 'language' in the hierarchy of classification either dialect or communalect are commonly used. However, recently, the term 'lect' has been adopted to capture the ambiguity between language and dialect and in part also to avoid the pejorative overtones of dialect.

Reconstructions and conventions

Reconstructions form a particular focus of historical linguistics, usually denoted by an asterisk (*) and often referred to as 'starred forms'. These are abstract forms, derived from attested languages that were supposedly part of a hypothetical proto-language. Thus an author citing * plus a formula for a word is implying that it was part of the proto-language spoken by the particular reconstructed group. Terms such as Proto-Indo-European are common enough to be standard terminology. However, not all authors use the same standards of evidence to derive these proto-forms. Problems arise

- when the dataset is defective, i.e. lexical attestations are known only from some languages in the proposed sub-group
- when a reconstruction is built indirectly, i.e. on the back of other reconstructed forms whose status is doubtful.

Proto-forms can be cited for defective datasets; this is an inevitable part of hypothesis building. Problems arise when speculative reconstructions of this type are quoted as solid results by specialists from another area.

In some domains of African language research a distinction has been adopted between a 'quasi-reconstruction' or 'pseudo-reconstruction' and a 'regular reconstruction' (e.g. in Bendor-Samuel 1989). Quasi-reconstructions are essentially well-informed guesses based on partial datasets as opposed to regular reconstructions which are based on a thorough analysis of historical sound-correspondences. Quasi-reconstructions are marked '#' in contrast to regular reconstructions which retain the asterisk *. This distinction is difficult to enforce as authors are inevitably touchy about the reality of their reconstructions. This is particularly true of deep-level reconstructions such as the hypothetical Nostratic; the claim by Hegedüs (Ch. 4, Volume I) that it is based on regular sound-correspondences would be disputed by many historical linguists. However, as variations arise in the reconstruction and sub-grouping of the language phyla of the world, historical linguists will gradually be compelled to become more critical of proposed reconstructions.

Phonetic characters and orthographic conventions

These books make no apology for making use of the technical conventions of linguistics; unless authors can back up their results in a way credible to linguists their assertions will remain speculative. As far as possible, authors have been encouraged to shift their data tables to an Appendix and to establish a clear flow of argument independent of these. The tables have been left in place, however, where argument and data are inextricably intertwined.

In an ideal world, all linguists would switch to a standard set of conventions for representing phonetic characters and these would be internationally agreed upon and developed or expanded as research continues. The conventions of the International Phonetic Association (IPA) largely serve this function in the case of basic phonetic research and often in the description of undescribed languages. However, where an old-established research tradition exists, as in Indo-European, Kartvelian or Sino-Tibetan, phylum-specific conventions have been established and writers are often loath to break away from these and shift their whole dataset to IPA. In addition, orthographies that have been developed in the twentieth century for mission or other literacy purposes, often reflect the technology of the period. Where authors were expecting to produce primers or Bible translations they developed conventions that were effective on typewriters. In some cases these have been well established and now that printed materials are produced by computer, word-processors have to mimic these.

In the chapters that follow most authors use IPA phonetic symbols, but in the case of well-established traditions, they follow disciplinary orthographic conventions. Where these might be obscure they are explained in endnotes.

Editorial policy

Approximately half the contributions in these volumes were written by scholars whose first language is not English. These books are not intended to present a façade of ideological homogeneity; indeed as an overview of the field, they include many contradictory points of view. A particular effort has been made to include research by Russian and East European scholars, the importance of whose work is only gradually being recognized. This has involved the editors in very extensive rewriting in places and it is not always easy to ensure that the full meaning of the original has been retained. An endnote following each chapter indicates the extent of the changes that have been made. Some of the flavour of Russians writing in English has been maintained, partly because it is also important to understand the parameters of what is a strikingly different style of argumentation.

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General introduction

ROGER BLENCH AND MATTHEW SPRIGGS

PRINCIPAL THEMES IN ARCHAEOLOGY AND LANGUAGE

The relationship between linguistics and archaeology has been affected by both the internal dynamic of the disciplines in question and external political and social trends. Many archaeologists still feel that archaeology and linguistics do not share much common ground; some of the reasons for that are internal to archaeology, others can be traced to the sometimes startling misuse of these linkages by earlier scholars.

The idea of a relationship between a linguistic prehistory and an archaeological prehistory is a seductive one, but in the past it has often led to dangerous liaisons. The data from both disciplines are open to constant reinterpretation as new evidence comes in and new models are adopted. Linguists or archaeologists who interpret their data by tying them to a particular statement of 'fact' one year for another discipline may well find that 'fact' discredited the next and the interpretation of their evidence undermined. If circularity of argument is to be avoided these two databases for constructing prehistory must be assembled quite separately, and only then, at a subsequent stage of synthesis, compared.

For many areas of the world, such as the Pacific and Africa, it is common for an overview of linguistic prehistory to be available before an equivalent archaeological picture has been produced. The need is not for the newly arrived archaeologist to completely ignore hypotheses of culture history derived from linguistic data but to treat them as just that, hypotheses which may or not provide a realistic model for a region's prehistory. An explanation derived solely from archaeological data may turn out to have greater explanatory power or the original linguistic model may provide a plausible narrative which encompasses adequately the evidence of both disciplines. In this latter case, the archaeological data are not so much explained by the linguistic, as consonant with it, as both are linked to the same broad social processes. They may not be, of course, in any particular case.

The comparison of archaeological and linguistic evidence has not proved very popular in the post-1945 era, not only because of the stigma derived from the misuse of both disciplines by the Nazis to construct their 'master race' ideology, but also because of flaws in the method of comparison. Theories of language affiliation were often developed without the use of a critical or orthodox methodology to reconstruct human history. Isolated archaeological observations were being explained by equally isolated linguistic ones.

Another reason that archaeology and linguistics have been kept apart has been because of internal developments in archaeological theory, particularly the trend of the discipline towards a sort of 'archaeology is archaeology is archaeology' position. This has acted to exclude data from multiple sources:

> Yet there is little general awareness of the value of combining the study of archaeological data with that of historical linguistics, oral traditions, historical ethnography and historical records although it is clear that many archaeological problems can be resolved in this way. . . . the resistance seems to come from the view, widely held by processual archaeologists, that their discipline must be based as exclusively as possible on the study of material culture.
>
> (Trigger 1989: 356)

Partly in response to earlier theoretical excesses, the 'sceptical' generation of post-war western archaeologists was extremely aware of the limitations of their discipline for reconstructing a rounded prehistory. In 1956–7, Glyn Daniel could write:

> We must alas, for the most part, keep the builders and bearers of our prehistoric cultures speechless and physically neutral. This may seem to you an unsatisfying conclusion. And so it is but then much of our prehistory is unsatisfying and difficult, tantalisingly meagre and sketchy. We can appreciate this and accept the limitations of prehistory along with its excitements.
>
> (Daniel 1962: 114–15)

Hawke's (1954) 'ladder of inference' was one up which archaeologists climbed with increasing fear of heights. Details of prehistoric technology could be learned, economy could be investigated with some success, but the higher rungs of prehistoric socio-political organization would always remain shaky, and an understanding of prehistoric ideology forever beyond the reach of a sensible archaeologist (Hawkes 1954). Trigger (1989: 327, 392) notes that despite the optimistic assertions of the 'new archaeologists' of the 1960s such as Binford (1962), the processualist agenda as it developed in subsequent decades has remained firmly on the lower rungs.

From the early 1980s onwards, increasing concern was expressed by archaeologists over the seemingly limited goals of processual archaeology. A variety of approaches, often lumped together as 'contextual archaeology', have returned again to the optimistic aim of earlier generations to construct a more rounded

prehistory. Attempting to identify past social and linguistic groupings is part of this project. As perhaps with all such developments in social and historical disciplines, this is reflective of broader changes in contemporary society rather than being internal to archaeology.

We are in a period of growing interest in 'roots'. When personal identities are under a bewildering array of pressures, the certainties of the past are looked back to for answers to the question 'who am I?' In justifying his interest in the old question of the origins of the Indo-Europeans, Colin Renfrew (1987) did not claim purely disinterested motives for wishing to know 'What songs the sirens sang':

> You may ask, who cares? What on earth does it matter what language was spoken by long-dead people? . . . But language and identity are closely linked and there are few things more personal than the language one speaks. Indeed language and national identity are today very widely equated. One's 'ethnic' affinity is often determined much more by language than by any identifiable physical characteristics, and elections are won or lost by Flemish or Walloons, bombs detonated by Welsh nationalists and Basque separatists, and massacres perpetrated in many parts of the world – most recently in Sri Lanka – on the basis of distinctions which are linguistic and cultural more than anything else.
>
> (Renfrew 1987: 2)

And so he feels it must have been in the past: 'if we are interested in the origins of the modern world, we must understand the nature of past societies; this includes the social organisation of these ancient peoples and their sense of self-identity, which brings us to the questions of ethnicity and language' (ibid.: 3).

Trigger (1989: 376) sees this interest in the past of specific groups of people as part of a growing humanist trend in archaeology, in opposition to the goals of neo-evolutionist processual archaeology which saw case studies of particular regions as merely testing grounds for general theories of human behaviour and cultural change. When carried out among Third World and other native peoples such archaeology can be seen as both neo-colonialist and insulting. As archaeologists have become more sensitized to the needs and aspirations of the peoples among whom they work, and whose ancestors they may be studying, they have responded by providing histories which are relevant to the lives of local populations and seek to answer the 'where do we come from?' questions which help anchor identity in a world in flux.

STREAMS IN LINGUISTIC PREHISTORY

Tlon, Uqbar, Orbis Tertius: fringe theories of linguistic affiliation

As the epigraphs on p. v indicate, the view that historical linguistics has something to contribute to the history of peoples has been present for more than

two centuries. Indeed Johnson appears to be already reacting to an aspect of historical linguistics that has often caused it to be regarded with the gravest suspicion by other disciplines; the tendency for some of its practitioners to develop unusual models of world prehistory based on apparent links between geographically remote languages.

One of the earliest theories to develop along these lines was the version of Amerindian history that claimed the inhabitants of the New World were the Lost Tribes of Israel. This interpretation was advanced as early as 1650 when Menasseh ben Israel published his account of the traveller Aaron Levi who reported that he had encountered Hebrew-speaking Amerindians in the mountains near Quito. This type of linguistics is often broadly referred to as Voltairean linguistics from his famous characterization, 'Etymology is a science in which the vowels count for nothing and the consonants for very little.'[1]

This type of theorizing, usually the province of amateurs, is frequently linked with bolder cultural hypotheses, that usually involve long-distance migration, and often have a religious or political agenda. It is easily caricatured and may often provide a well-founded excuse for archaeologists and prehistorians to avoid this type of excursus. Such theories are, of course, not exclusively based on linguistic evidence, but lexical connections are always claimed to support comparison of material culture. Two key themes of this body of scholarship relate to specific regions of the world, Ancient Egypt and the Pacific.

The notion that civilization was somehow invented in Ancient Egypt and spread out through the remarkable navigations of its inhabitants has a pedigree as far back as Classical Greece (Bernal 1987), and the ascription of Egyptian origins to African peoples was well under way by the beginning of the twentieth century. Johnson (1921 but manuscript prepared in 1897) wrote an influential history of the Yoruba, arguing against an Arabian origin for the Yoruba and promoting their migration from Egypt. Such theorizing continues in the 1990s in the works of the followers of Cheikh Anta Diop and is often promulgated in luxuriously produced handbooks of hieroglyphics. However, claims for such land migrations were relatively restrained compared with the deepwater navigation proposed in classics such as Perry's (1923) *Children of the Sun*. Elliot Smith and later Thor Heyerdahl were eloquent proponents of long-distance migrations and much curious scholarship was adduced in support of such hypotheses.

There has been a substantial literature on pre-Portuguese trans-Pacific contact originating as early as the seventeenth century (Wauchope 1962: 83ff.). Although recent DNA research may be taken to suggest that such contacts did indeed occur at least sporadically, this is far from accepting that some of Kublai Khan's ships were driven eastwards to the New World after a failed invasion of Japan (Ranking 1827), or that fragments of the fleet of Alexander the Great reached the Americas in 323 BC (Gladwin 1947).

Exponents of such ideas are typically aggrieved when the predictably cautious academic establishment fails to take on board their ideas. One of the advocates of trans-Pacific contact took a robust view of their caution:

> All the lights in the House of the High Priests of American Anthropology are out, all the doors and windows are shut and securely fastened (they do not sleep with their windows open for fear that a new idea might fly in); we have rung the bell of Reason, we have banged on the door with Logic, we have thrown the gravel of evidence against their windows; but the only sign of life in the house is an occasional snore of dogma.
>
> (Gladwin 1947)

There is probably a useful distinction to be drawn between fringe ideas that draw the attention of more cautious scholars to possible previously unsuspected connections and similarities (Heyerdahl, for example), and those which are nothing more than an encumbrance to scholarship (Atlantis, Von Daniken, Velikovsky). Keep the windows open but look out through them rather than simply sleeping by them.

Links with nationalist ideologies

One of the more troubling aspects of the history of this discipline has been its links with nationalist ideologies. Linguistic nationalism still engenders a rich emotional harvest at present, often for good reason, since the suppression of minority languages is commonly a prominent feature of totalitarian governments. Democracies sometimes encourage voluntary euthanasia among minorities through neglect. None the less, when a national language is linked to a national culture it is a short step to linking that to archaeological entities and thence to broader historical claims on territory and political authority (see Kohl and Fawcett 1995).

Throughout the nineteenth century, these ideas would have been considered acceptable by many researchers and the links between nationalist ideologies and scientific research were unproblematic. However, somewhere in the early twentieth century, a split developed between the rationalist, academic tradition and the promotion of certain types of archaeology in support of nationalist goals. This has been well documented in Germany and the former Soviet Union, where linguistic ideologues developed theories of the relation between particular language groups and specific types of material culture and were ruthless with those tempted to disagree (Trigger 1989). None the less, evidence is mounting that there is a European-wide tradition of rewriting the past in pursuit of nationalist goals (Díaz-Andreu and Champion 1996).

Nikolay Marr (1865–1934) has been called the 'Lysenko of anthropology' in Russia and had a comparable influence on all types of linguistic, ethnographic and archaeological research in his tenure as Director of the Russian Academy of Material Culture. His career and influence are described in Slezkine's (1994) account of Russian imperial relations with the minority peoples of Siberia. Central to Marr's ideas were evolutionary or 'Japhetic' theories of language, whereby languages developed in stages from 'primitive' to advanced. Primitive societies had 'mollusc-like' speech forms which had to develop 'upwards', until

at the conclusion of history all language would merge into a single Communist speech. This eventually led him to the conclusion that both ethnography and archaeology were anti-Marxist and these were formally condemned at the All-Russian Conference on Archaeology and Ethnography in 1932. The practical consequence of Marr's tenure of authority was the destruction of much of the academic infrastructure around these subjects: museums, journals and learned societies were disbanded and non-Marxist teachers persecuted. Marr's work was explicitly rejected by no less a figure than Stalin, who wrote an essay in 1950 examining the relation of Marxism to linguistics (Slezkine 1994: 314). Shnirelman, who describes the Russian 'linguoarchaeology' in Chapter 10 of Volume I, warns that links with nationalist ideologies are still alive in the 1990s although their structure is less formalized than in an era of centralized State control.

German linguists played an important role in the development of Indo-European scholarship and as early as the mid-nineteenth century Jacob Grimm was to explain the distribution of various sound changes by referring to the ethnic character of speakers. Gustaf Kossinna (1858–1931), whose principal work, *Die Herkunft der Germanen*, published in 1911, became a key text in Nazi Germany, provided an important ideological plank for territorial expansion. He argued that specifically Germanic material culture could be identified in archaeological sites and that where such material was found, this was evidence of the original extent of Germany.

The positivist tradition

It is tempting to dismiss both marginal historical linguistics and nationalist ideology as forgotten errors of a past epoch. However, they have had an important historical influence on archaeologists, making them wary of all types of correlation with linguistic theories, no matter how carefully couched.

Another more sceptical tradition of historical linguistics has existed for several centuries and indeed persisted through a long period of neglect. For example, precursors to historical linguistics exist both among the Sanskrit grammarians and in the works of the rabbinical grammarians. Most striking is the work of Yehuda Ibn Quraysh, who lived in Fez, Morocco in the tenth century and was the first to compare the phonology and morphology of Hebrew, Aramaic and Arabic in his book *ʿRisāla* (Téné 1980). Such precursors seem to have had little influence on their successors and an intellectual tradition only developed after historical linguistics was put on a more scientific footing. This event is conventionally attributed to Sir William Jones's famous lecture in 1786 demonstrating the links between Sanskrit and the Classical languages of Europe, but it has become clear that Jones's perception was far from original. Bonfante (1953) quotes a reference to an unpublished manuscript by Marcus Boxhorn (1612–53) hypothesizing a 'Scythian' origin for all the major languages of Europe, while in Saumasius's *De Hellenistica*, published in 1643, reconstructed proto-forms for European numerals are proposed. The concept of reconstruction of an Indo-European proto-language appears as early as 1713 in the works of the English divine, William Wotton:

> My argument does not depend on the difference of Words, but upon the Difference of Grammar between any two languages; from whence it proceeds, that when any Words are derived from one Language into another, the derived Words are then turned and changed according to the particular Genius of the Language into which they are transplanted. . . . I can easily suppose that they might both be derived from one common Mother, which is, and perhaps has for many Ages been entirely lost.
>
> (Wotton [1713] 1730: 57)

Wotton had related Icelandic ('Teutonic'), the Romance languages and Greek, which are certainly as convincing a demonstration of Indo-European affinities as Jones's demonstration of the links of classical languages with Sanskrit. Moreover, Wotton developed some estimates of the speed of language change and was concerned about the apparent contradiction with the widely accepted 'Biblical' age of the earth. Jones, in contrast, erroneously believed that Egyptian, Japanese and Chinese were part of Indo-European while Hindi was not, which suggests that his method was flawed.

Outside Indo-European, Uralic classification had been virtually completed prior to Jones. As Ruhlen observes:

> The basic structure of the Uralic family had thus been roughly worked out at least six years before William Jones's celebrated address, which opened the era of [Indo-European] studies.
>
> (Ruhlen 1991: 66)

The nineteenth century was a major period for the development of historical linguistics and indeed most of the debates which still characterize the discipline in the 1990s have their origin in the work of scholars of the previous century. Throughout the nineteenth century, there was a strong conviction that language could be analysed to establish historical results. Donaldson commented in the 1830s:

> There is in fact no sure way of tracing the history and migrations of the early inhabitants of the world except by means of their languages; any other mode of enquiry must rest on the merest conjecture and hypothesis. It may seem strange that anything so vague and arbitrary as language should survive all other testimonies, and speak with more definiteness, even in its changed and modern state, than all other monuments however grand and durable.
>
> (Donaldson 1839: 12)

and Craik in the 1860s:

> Each language has a life of its own, and it may be made to tell us its own life, so to speak, if we set the right way to work about it.
>
> (Craik 1861: 1)

Just as Finno-Ugric (i.e. Uralic) and Indo-European were earliest on the scene in terms of historical reconstruction, so they began the tradition of reconstructing history through lexical reconstruction. Early attempts to do this, such as Pictet (1859–63),[2] evolved convoluted theories of the migrations of the Aryan race that we should now consider highly suspect; however, this should not distract attention from the significance of the enterprise.

These efforts continued throughout the late nineteenth century and they served to establish the conventions which were to be adopted and developed elsewhere in the world. Historical linguistics of this type requires a certain density of research to be credible; without adequate lexical materials for language classification and reconstruction no amount of methodological sophistication will fill the lacuna.

The pattern of research

Research concentrations are often reflections of political accessibility and funding. Research on the Andamanese and Nicobarese languages has remained largely static due to the refusal of the Indian government to issue research permits. Although they coexist in the same part of the world, Papuan has lagged far behind Austronesian due to the inaccessibility of many Papuan languages. Comparative Australian has taken off following the efforts of relatively few highly motivated individuals. Bantu is far better known than Niger-Congo due to early interest in the topic, accessibility of many of the languages and relatively unproblematic transcription.

Despite these problems, a global picture of the disposition and relations of language phyla is slowly beginning to emerge. The pattern of phyla in the world appears to be relatively stable (although the analysis of macrophyla is highly controversial: see next section). Data are beginning to be less of a problem than collating them. Few regions of the world are entirely without archaeology, although the density of excavated sites is highly variable. In consequence, crackpot theorizing and promotion of nationalist ideologies are at a lower level and the volume of papers and books exploring the links between language and archaeology is on the increase. The major threat to this area of scholarship is probably now its allegiance to an old-fashioned empiricism and a positivist commitment to data; to avoid strangulation at the hands of the post-modern devotees of Kali it will have to develop more sophisticated public relations. Lenin is reputed to have said that the express train of history cannot be stopped; all that revolutionaries can do is grease the wheels.

THEMES IN THE INTERACTION OF LINGUISTICS AND ARCHAEOLOGY

Historical linguistics, lexicostatistics and glottochronology

The single most important theme of these books is the interaction of historical linguistics with archaeology. Historical linguistics may be defined as the analysis

of the relationship between two or more languages that are assumed to be genetically related, that is to 'have sprung from some common source', such as English and German. Linguists are concerned to develop testable rules by which specific languages can be related to one another, relating to phonology, morphology and lexicon. These rules generate a tree-like genetic structure which allows the modelling of the relative antiquity of splits between different languages or other more complex aspects of their interrelations (see Ross, Ch. 13, Volume I). Proto-forms predicted by the rules that relate two or more languages and a sequence of proto-languages can be reconstructed for nodal points in the genetic tree.

Lexicostatistics, the counting of cognate words in a standardized list, and assigning a numerical degree of relationship seems to have been first used in the early nineteenth century. Dumont d'Urville (1834) compared a number of Oceanic languages (which would now be called Austronesian) and proposed a method for calculating a coefficient of relationship. He extended his comparison to some Amerindian languages and concluded that there was no evident relationship with the Oceanic languages in his sample. Hymes (1983) provides a detailed history of the further development of lexicostatistics in the nineteenth and twentieth centuries.

Another aspect of historical linguistics is glottochronology. Writers such as Wotton ([1713] 1730) believed that it was possible to calculate how rapidly languages change by comparing ancient texts of known date with the modern form of languages. Robert Latham (1850) was probably the first author to sketch the possibility of assigning a precise date to the split of two languages through applying a mathematical algorithm. Hymes (1983: 73ff.) cites other tentative experiments in the nineteenth century but these seem not to have been developed until Swadesh (1952).

Lexicostatistics and glottochronology have the attractive aspect of quantification; they seem to represent a scientific approach to the dating and genetic classification of languages. However, very few historical linguists now accept the premises of such approaches. In part this may reflect a wave of criticism of the mathematics underlying these methods (see discussion in Hymes 1983: 75). More important, however, has been the realization that languages undergo a variety of changes in interacting with one another. Lexicostatistics must assume a standard of lexical purity that allows languages to change at a regular rate especially in their core vocabulary. Using the methods of historical linguistics that are generally accepted, only relative dating is possible; for absolute dating linguists now turn to archaeology.

Historical linguistics as a discipline

Whereas archaeology is taught as a method that can be applied to any situation, rather like economics, and although archaeologists divide into theoretical schools and schools develop their own terminologies, this is usually not location-specific. Indeed, within a single institution different methods may well be propagated by individual scholars. In other words, the archaeology

of Japan or Australia does not appear to have a large technical vocabulary that would not be immediately comprehensible to a regional outsider.

Although theoretical linguistics has many of the same intellectual subdivisions there is only a limited interface between historical linguists and the larger linguistic establishment. This is partly because historical linguistics remains a minority interest in a world dominated by syntax, phonology and to a lesser extent, socio-linguistics. Historical linguists are often partly self-taught or take their cue from individual teachers. The consequence is that there can be striking disagreements over method and standards of evidence; this debate is most apparent in the case of the sometimes bitter disputes that have ranged over macrophyla (see pp. 13–15).

Scholars of the older-established phyla often take a patronizing attitude to results from those phyla more recently recognized. This is particularly striking in the case of Indo-European where the conviction that the phylum is well founded and that its reconstructions are accurate and convincing appears to be widespread among its adherents. A darkly humorous version of this can be seen in the comments of Hopper (1989), reviewing Thomason and Kaufman, who contrasted the 'factually established genetic categories' such as Indo-European with 'broad-based guesses' such as Niger-Congo, Afroasiatic and Nilo-Saharan. The view taken in these volumes is that the major language phyla of the world that are accepted by the scholarly community are all equally well founded.

The Indo-Europeanist habit of ignoring what are strangely called 'minor languages' has resulted in a virtual lacuna in research on Indo-European languages of India with only small numbers of speakers. One of the more evident tendencies in Indo-European linguistics is to give primacy to written languages, such as Sanskrit. Thus, reconstruction of the Indo-Aryan languages is in terms of relating the present-day forms to attested Sanskrit (cf. Turner 1966) rather than subjecting the body of Indo-Aryan languages to the usual procedures of historical linguistics. The consequence has been a striking inadequacy of fieldwork to describe the more than 300 unwritten Indo-European languages spoken in the India–Pakistan region in the 1990s (see the assessment of research needs in Grimes 1992). The conventional practice of historical linguistics in the region is thus in a rather backward state. Applying the standards of proof common, say among Austronesianists, would of course reduce Indo-European to a 'broad-based guess'.

Geographical coverage

All types of research have a patchy coverage when considered globally, but linguistics and archaeology have proven especially sensitive to political and economic constraints (see p. 8). Different disciplinary traditions also lead to uneven emphases with particular regions. For example, although East Asian archaeology is well represented in terms of excavated sites, specific digs seeking the origins of food production are a relatively new phenomenon. The incidence of monuments can be in inverse relationship to an emphasis

on economic prehistory. Countries with a dominant culture often discourage work on regional languages for fear of encouraging local aspirations. Until recently, the languages of China, were poorly known, and research on minority languages unaccountably spoken by peoples not part of an officially recognized minority was strongly discouraged (Ramsey 1992: 162ff.).

In addition, intellectual traditions and the organization of scholarship affect interdisciplinary work. Countries with national research centres that unite scholars from different intellectual areas, such as France, the former Soviet Union and Australia, are far more likely to produce interdisciplinary scholarship than England and America, where experts are ghettoized in university departments. Generally speaking, where careers depend on publications, and only publications in a specific discipline are highly valued, there is every incentive to concentrate in one intellectual area to the exclusion of others. Indeed, in both linguistics and archaeology, intellectual justifications for excluding other approaches have been explicitly developed, as witness the example of generativism (Chomsky 1988).

The consequence has been that both historical linguistics and its combination with archaeology are developed to very different degrees in different parts of the world. The areas where the focus has been most significant are Eurasia and Oceania; Eurasia because of the Indo-Europeanist tradition and its remarkable survivals in the former Soviet Union, and Oceania because of fortunate support for this type of approach in a few key institutions. India represents a curious lacuna in Eurasia since, despite its importance in the early decades of the twentieth century and the production of the massive 'Linguistic Survey of India' during the 1920s, restrictions on research permits have led to an almost complete cessation of research by outside scholars on its some 500 unwritten languages. The New World and Africa have been marked by a relatively small amount of research. In Africa this may be due to nothing more than time-depth (convincing amounts of data have only recently become available) and lack of dedicated institutions. In the case of the Americas, however, despite the all-embracing tradition of anthropology which conjoined archaeology, cultural anthropology and linguistics, the absence of a major tradition of synthesis suggests that the reality has been academic isolationism.

Texts and pretexts

One of the earliest interfaces between archaeology and language has remained distinct from the type of historical linguistics discussed here: the interpretation of ancient written documents and the decipherment of scripts. This story has been rehearsed too many times (e.g. Simpson 1985) to need further recounting, beginning with the decipherment of hieroglyphics and cuneiform, through to Hittite and other epigraphic languages of the Ancient Near East. In the twentieth century, decipherment has been extended to India, China and Central America and continues in the 1990s with recent proposals for the decipherment of the Olmec script of the Yucatan (Wichmann, Ch. 13 in this volume). Epigraphy is also equipped with its own eccentric fringe;

a Harvard professor of zoology tells us that inscribed rocks in Texas record the journey of migrant Zoroastrians from Iberia some 2,000 years ago (Fell 1980: 164).

Interpreting epigraphy and relating it both to known historical events and to excavated sites has been a major theme of archaeology, especially in the Near East. Indeed, the prominence accorded to written texts has obscured other types of interpretation of linguistic data. Thus a considerable body of work exists translating, transcribing and interpreting ancient texts in a variety of Semitic languages; overall models of the evolution and dispersal of this language family barely exist. An example of this is the attempt by Zohar (1992) to interpret the spread of Semitic in the Near East. African Semitic languages (which are considerably more numerous and diverse than those of the Near East) are referred to as 'minor languages' in the text and excluded entirely from the family tree of Semitic (Zohar 1992: Fig. 1).

There is a strong argument for supposing that much of the most innovative work in using historical linguistics has been brought about by the *absence* of ancient texts. Just as North American archaeology developed innovative analytic techniques to analyse the sites of hunter-gatherer communities, modelling in historical linguistics has been stimulated in regions of the world where there are no early texts.

Testable hypotheses

One of the attractive aspects of linking historical linguistics with archaeology is that it is possible to generate testable hypotheses. Linguists are usually way ahead of archaeologists in their speculations. Finding an informant for a language is easier and far less costly than mounting an archaeological expedition to search, for example, for the origins of food production. An experienced linguist can often elicit a range of basic and key cultural vocabulary in a few hours, whereas excavations often take many months and sometimes years. Historical linguists are often tempted to throw off hypotheses on the origins of food production far more quickly and perhaps more casually than would be permissible within other academic frameworks.

However, when a prediction is made then it can at least be tested. So, for example, if an historical linguist claims that certain species of domestic animal can be reconstructed back to the proto-language of a particular phylum, and at the same time makes a proposal for the homeland of the speakers of the proto-language, then excavations should ideally be able to confirm the presence of those species. An example of such a correlation is presented in the chapter by Green and Pawley (see Volume III) where linguistics is used both to pinpoint a proposed homeland of Oceanic languages and to suggest the features of house-forms that should be present. Archaeology suggests that house-structures of the predicted type are indeed present. Such correlations are rare in practice especially when only a small number of sites have been identified, but as the density of well-investigated sites increases, hypotheses can be subjected to a reasonable test.

Phyla and macrophyla

There are some language phyla whose existence is generally accepted, such as Indo-European or Austronesian, as a result of the weight of scholarly opinion. In a few cases, such as Nilo-Saharan, a body of scholarly comment exists, questioning either its unity as a phylum or the families that compose it. In addition, there are regions of the world where a large number of languages exist which show common features but which have not been shown to be related to the satisfaction of most researchers. These 'geographical' names are often shown as phyla in works of synthesis. The most important of these are Papuan, Australian and Amerind; groups of languages whose relationship can be demonstrated but where overall genetic relations have proved resistant to the methods of historical linguistics. Similarities of phonology or other features do suggest a common origin; but it is possible that they have so far diversified from a common proto-language that proof will remain a chimera. Finally, in one case, Andamanese, inadequate data make any final judgement impossible at present. Table 2 sets out the language phyla of the world and their status in this hierarchy.

Since the 1980s, numerous publications have advanced the case for macrophyla, that is, the uniting of several accepted phyla into one genetic group. The best known example is Nostratic, a macrophylum that brings together most of the phyla of the Eurasian land mass, whose membership varies according to different authors. The journal, *Mother Tongue*, has published the speculations of 'long-rangers' who wish to promote continent-spanning comparisons. With increasing awareness of the traditions of such scholarship in the former Soviet Union and the publication of some major texts (e.g. Bomhard 1994) this type of large-scale comparison has reappeared. Other more controversial proposals include Indo-Pacific and Amerind (Greenberg 1987) and Sino-Caucasian from the Soviet School, especially Starostin (e.g. Shevoroshkin 1992). These proposals have excited considerable scepticism although most linguists do not command the vast range of data that would be necessary to give them a full evaluation. Ruhlen (1991: 270ff.) gives a lengthy bibliography of 'alleged connections between families usually assumed to be unrelated' which suggests that almost any two or more of the world's language phyla have been related by some researcher.

Behind such enterprises is an intriguing and controversial agenda; the reconstruction of proto-World or 'Proto-Sapiens' as Ruhlen (1994: 192) has it. The hypothesis that all human language has a common origin is certainly emotionally persuasive; the myth of the Tower of Babel still exerts a powerful pull. However, conviction is not proof and enthusiasm not demonstration. Although one of the most eloquent advocates of proto-World, Vitaly Shevoroshkin, has recited poems in this remarkable language on radio and television, this cannot yet conjure it into reality.

The exploration of long-range comparison has aroused considerable opposition; historical linguists working on a smaller scale are frequently outraged at the misuse of language data by non-specialists. Trask (1995), for example, has analysed in considerable detail the evidence for a traditional hypothesis

Table 2 Language phyla of the world and their status

Phylum	*Usual acronym*	*Where spoken*	*Status/comment*
Niger-Congo	NC	Western, Central and Southern Africa	Accepted
Afroasiatic	AA[a]	Northeast Africa and the Middle East	Accepted
Indo-European	IE	Eurasia	Accepted
Uralic	U	Eurasia	Accepted
Kartvelian	K	Caucasus	Accepted
North Caucasian	C	Caucasus	Accepted
Chukchi-Kamchatkan	CK	Siberia	Accepted
Yeniseic	Y	Siberia	Accepted
Dravidian	DR	India	Accepted
Sino-Tibetan	ST	Central Asia	Accepted
Miao-Yao	MY	China	Accepted
Daic (=Tai-Kadai)	D	Southeast Asia	Accepted
Austroasiatic	AS[a]	Southeast Asia	Accepted
Austronesian	AN	Pacific	Accepted
Eskimo-Aleut	EA[b]	Bering Strait	Accepted
Pama-Nyungan	PNY	Australian	Accepted
Na-Dene	ND[b]	North America	Accepted
Khoisan	KH	Eastern and Southern Africa	Usually accepted
Nilo-Saharan	NS	Eastern and Central Africa	Usually accepted
Altaic	AT	Eurasia	Usually accepted although the affiliation of Korean is debated
Papuan	PP[b]	Papua	Consists of a large number of accepted groups but their unity is not considered proven
Australian	AU[b]	Australia	Consists of a large number of accepted groups but their unity is not considered proven
Amerind	AM[b]	Americas	Consists of a large number of accepted groups but their unity is not accepted
Andamanese	AD[b]	Andaman islands	Inadequate data make effective historical linguistics impractical

Notes: This table excludes a number of well-known isolates such as Basque, Burushaski, Ghilyak, Ainu and Japanese, as well as African isolates (see Blench, Volume III), and problematic languages of Asia such as Nahali and Kusunda.

a AA is unfortunately used for both Afroasiatic and Austroasiatic. AS is adopted here for Austroasiatic to eliminate confusion. PN is applied to Polynesian, hence the use of PP for Papuan here.

b Proposed acronym.

linking Basque to Caucasian languages and concludes that it depends in almost every case on a misuse or defective analysis of the Basque language materials. Thurgood (1994) has shown that the hypotheses, such as Benedict's Austro-Tai, that link together the major language phyla of Southeast Asia are based on ancient loanwords.

Between near-global hypotheses and accepted phyla stand more modest proposals that link together two phyla that already have a history of observed similarities. Two are Austric (linking the Austronesian and Austroasiatic phyla: Reid 1994) and Niger-Saharan (Niger-Congo with Nilo-Saharan: Blench 1995). The linking of Japanese (or Japonic) to the Altaic phylum has a venerable pedigree but still generates controversy and cannot be regarded as accepted.

Intriguing as these planet-spanning proposals are, they remain to be critically evaluated by the body of historical linguists and thus cannot easily be used by archaeologists. Indeed there are still few wholly convincing models to explain the origin and diversification of accepted phyla; to interpret the more doubtful macrophyla would be over-egging an already rich pudding.

Linguistics and genetics: 'The New Synthesis'

An aspect of the reconstruction of prehistory that has come to the fore since the mid-1980s is the use of evidence from genetics, especially from analysis of mitochondrial DNA. However, the reputations of traditional biological anthropologists have stood at an all-time low following analyses such as that of Gould (1982) who accurately skewered the underlying racial preoccupations of the supposedly scientific physical anthropologists of the nineteenth and early twentieth centuries. It should be noted that osteometrics remain acceptable in many European traditions, especially in France, as witness a standard text on human remains in the Sahara (Dutour 1989).

However, a major break with traditional biological anthropology occurred with the development of modern techniques of DNA analysis; both because DNA could potentially be recovered from archaeological material and because DNA analysis seemed to offer a way of relating present human populations to one another and to past materials. Linguistic classifications of human populations seemed to offer a way beyond simple racial models; more abstract, they seemed to provide an ideal analogue to the classificatory trees from DNA. If DNA trees and language trees were to correspond then this would provide striking mutual confirmation for models of human prehistory. Indeed the links between them were enthusiastically promoted in the late 1980s and into the early 1990s as 'The New Synthesis' (see, for example, Cavalli-Sforza *et al.* 1988; Renfrew 1992). The culmination of this trend was the appearance of *The History and Geography of Human Genes* (Cavalli-Sforza *et al.* 1994) which promotes a major revision of the methodology for exploring human history.

Some archaeologists are among those disturbed by the implications of the New Synthesis for encouraging narrow nationalistic readings of history, and restoring the discredited view of race, language and culture as generally coterminous (Pluciennik 1996). Linguistic and archaeological naïvety aside, the

new data of genetics are not being added in a political vacuum as geneticists sometimes seem to assume. A more self-critical awareness is clearly required when dealing with the implications of broad genetic generalizations linked most uncertainly, as Pluciennik points out, to archaeological entities.

Such entities themselves are sometimes subject to divisive claims by putative descendant groups. For instance, the ongoing dispute over who are the 'real' Macedonians with a claim to the heritage of Alexander's symbols of power nearly brought Greece and 'The Former Yugoslav Republic of Macedonia' to war (Brown 1994). In such circumstances genetic data are more than likely to be seized upon and misused to stir up feelings of enmity between the rival claimants. Language and archaeology already have been misused in this way.

More recently there has been a distinct withdrawal from some of the claims of this type of work. The 'fit' between language trees and DNA results has been seen not to be quite as close as suggested in earlier publications. Chen, Sokal and Ruhlen (1995: 610) compare genetic and language trees on a global basis and conclude: 'The consensus between language trees and genetic trees is low . . . so low as to make the trees incomparable.'

This will probably remain the case on the scale of phylic and macrophylic relations which they analyse. With very large land masses such as Eurasia, language shift is an extremely common process, as the disappearance of Basque-related languages suggests. To find a people speaking their 'original' language may prove to be the exception. In contrast, much of the Pacific has seen expansion of populations into otherwise uninhabited territory. Almost certainly Oceania will again prove an important testing-ground for the methods of DNA analysis as it has with linguistics and archaeology, because the parameters of population movement and contact can be simplified.

CONCLUSIONS: AN AGENDA PAST 2000

With the publication of these volumes, we hope that the process of synthesizing historical linguistics and archaeology will have largely shaken off its previously negative image. There still remain many archaeologists who would seem to hold the view, either explicitly or implicitly, that linguistic and human biological evidence are either inadmissible or irrelevant in the discussion of archaeologically defined entities such as 'cultures'. At one level they are of course right. Much confusion has occurred in the past by the mixing at too early a stage of investigation of concepts and terms between disciplines involved in researching the history of particular regions. If, however, it is history one is after, rather than simply a narrow archaeology, then archaeologists cannot ignore important sister disciplines such as historical linguistics, genetics and human biology when attempting to synthesize the evidence.

An encouraging trend, represented by the interest shown in the language and archaeology sessions held at the New Delhi WAC Congress, is the increasing

number of linguists and archaeologists who are interested in what multidisciplinary research has to offer. The potential to construct and distribute large databases of linguistic information has made confidence in the broader results of historically minded scholars more credible. At the same time, the allure of results from DNA analyses and other types of genetic data has the potential to attract those seeking the credibility of 'hard science'. Although such eclecticism has a certain congruence with post-modern positions, the potential testability of hypotheses should align it more with an empiricist platform.

We must remain aware, however, of the abuses of the earlier part of the twentieth century, when archaeological, linguistic and physical anthropological data were combined wilfully to create extreme nationalist fantasies that race, culture and language are always coterminous. There are enough examples of this from recent and indeed contemporary history to necessitate us being critically self-aware of how our interpretations could come to be used in ways which we never intended, by people whose views we may not wish to subscribe to. The alarm bells sounded in some quarters over the 'New Synthesis' of archaeology, genetics and language need to be heeded.

As with all types of scientific changes, paradigm shifts occur over time, though with a less revolutionary time-scale than that advocated by Kuhn (1962). Universities and academic institutions have been able to keep dominant schools of method coherent through control of publishing and because a relatively small circle of individuals was in power. As these networks of power increasingly fragment, as publishing becomes cheaper and more accessible (particularly with advances in information technology) and as more research takes place outside the academy, then more diverse approaches to interdisciplinary studies will be able to flourish. With this added diversity of approach, it can be predicted that the current unfortunate distinction between prehistory and history should lessen or even disappear. Both the study of the archaeological evidence of the past and the modelling of social change through historical linguistics should be considered valid approaches to the past. The result should be the study of the broad outlines of a human history which allows for a complexity in the past that is so evident in the present.

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NOTES

1 Although quoted in Leonard Bloomfield's *Language* (1935: 6), the direct source in Voltaire's writings has yet to be uncovered and there is more than a suspicion that this is a piece of convenient linguistic folklore.
2 Pictet also first used the expression 'linguistic palaeontology', often attributed to more recent authors.

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Introduction to Volume II
Correlating Archaeological and Linguistic Hypotheses

MATTHEW SPRIGGS AND ROGER BLENCH

This volume consists in the main of substantive case studies where attempts are made to compare cultural sequences derived independently from archaeological and linguistic data. In some cases genetic and other biological evidence is brought in as well. The independence of these lines of evidence, only brought together after initial conclusions have been drawn on the basis of research conducted within the separate disciplines, is crucial here. This point has been stressed in the General Introduction to these volumes and is underlined again in this volume by Ross (Ch. 6), among others. Part of the difficulty of convincing archaeologists in particular of the value of linguistic or genetic evidence has been a tendency to circularity of argument. As MacDonald notes in Chapter 1 of this volume, there are many examples of linguists relying on archaeological data to provide an absolute chronology for their theories and archaeologists then citing the linguistic chronology to support their own.

MacDonald's chapter begins Part I, 'Correlating archaeological and linguistic sequences', and is a classic exposition of how such circularity can be avoided in presenting first the archaeological sequences and supporting evidence for a particular region, and then relating that to independently derived linguistic and biological evidence. One point he makes early on in his chapter is that archaeological evidence extends further back in time in most regions than standard historical linguistic models can hope to reach. The point at which a linguistic history of a particular phylum or postulated higher order grouping can be used to provide a comparative perspective is problematic in the absence of an overarching theory linking language spreads and archaeological phenomena.

Without such a theory one is left with purely distributional evidence. Archaeological phenomenon X is distributed over area A at a particular time. The distribution of language family Z as reconstructed using historical linguistic methods encompasses area A. Therefore Z relates in some way to X. This may work well in areas of the world with a comparatively uncluttered cultural history, such as in parts of the Austronesian-speaking area, but for many continental areas with a long succession of cultures occupying any

particular region, the resulting 'kaleidoscope' pattern of languages, as Higham calls it (Ch. 3), makes seeking meaningful patterns of correlation very difficult.

One broad-brush theory linking the spread of language and archaeological phenomena on the broad-scale has been developed by Renfrew (1989, 1992, 1996, also Volume I, Ch. 6) and Bellwood (1991, 1993, 1994) from their earlier researches attempting to correlate the distributions of Indo-European languages (Renfrew 1987) and Austronesian languages (Bellwood 1975, 1996) with regional archaeological sequences. They remark on the existence of large-scale language phyla across areas where archaeological research has identified important episodes of migration of relatively homogeneous cultures associated with the origins and spread of neolithic agriculture. Several of these phyla seem to have originated in areas adjacent to early centres of domestication of plants and/or animals: the Middle East, East Asia, West Africa and parts of Meso- and South America.

The purported homogeneity of early agricultural cultures in these areas suggested to Renfrew and Bellwood that their spread was a result in the main of actual population movement rather than simply technological diffusion (although at the local scale such processes may have been important). If so, these language phyla cannot have spread by processes such as language shift among already settled groups or development of widespread pidgins or lingua francas. They consider such processes as unlikely mechanisms for language spread over wide areas in pre-state societies and the only process which can explain large-scale linguistic entities such as some of the world's major language phyla is actual migration of peoples with an agricultural technology into areas relatively sparsely settled by hunter-gatherer groups.

Although MacDonald tentatively pushes archaeological and linguistic correlations in West Africa back to the Terminal Pleistocene, his confidence in the possibility of meaningful correlation increases considerably when pastoral and/or agricultural expansions from 5000 BP onwards are considered. Working in an even more linguistically complex continental area, van Driem (Ch. 2) explicitly links early Tibeto-Burman language dispersal to a neolithic rice-agricultural horizon traceable back to the Upper Yangzi area of Sichuan some 8,000 to 9,000 years ago. This is part of the Chinese agricultural centre identified by Bellwood (1994) as a potential homeland of several phyla. As well as Sino-Tibetan (sometimes called Tibeto-Burman),[1] he suggests Austroasiatic, Austronesian, Tai-kadai and Miao-Yao as originating in this centre.

A broad-brush approach is necessitated by the undeveloped state of archaeology in much of the region, particularly towards the west in the Himalayan area. Van Driem laments the lack of any absolute chronology in many areas critical for his hypotheses. The archaeology he draws on is thus in a comparatively early stage of elucidation, necessitating reliance on distinctive 'marker fossils' in the archaeological record, traits such as shouldered and tanged axes or dog burials, rather than relying on well-rounded descriptions of archaeological cultures. One is reminded of the state of play in Southeast Asian archaeology through to the 1950s, with archaeological cultures defined

primarily on axe typology, following the work of Heine-Geldern (1932, 1945) and others.

With the growth of archaeological research in Southeast Asia, great parts of the old paradigm have been overturned and a more textured prehistory has emerged. As archaeological research proceeds in the Sino-Tibetan linguistic area, it will be interesting to see how well van Driem's hypotheses hold up. We would stress that they will fare better than Heine-Geldern's, for two reasons. The first is the linking of archaeological and linguistic data to a plausible overarching theory of language and cultural expansion which seems to have wide applicability. The second is that key parts of his hypotheses do involve areas where a more nuanced archaeology is available, where relatively complete cultural sequences have already been established, for example North China. Such sequences serve to anchor van Driem's more tentative correlations to those in better-known areas where it is possible to make informed judgement on the plausibility of the proffered correlations.

Higham (Ch. 3) also provides a general synthesis of a partly overlapping area, but his vision extends further south into other areas of Southeast and East Asia. His general sympathy is for a similar explanation to van Driem's, linking agricultural expansion to present-day linguistic distributions. Again the starting point is a proposed expansion from a neolithic centre of rice agriculture in the Middle and Upper Yangzi Valley of China in the last 8,000 years. He traces an aspect of the early southward expansion of this horizon, rather than the southwestern and northeastern movements examined by van Driem for the same general time-period.

A particular problem for the Southeast Asian area is that large lowland areas which may have been occupied by neolithic groups at the beginning of that expansion were progressively flooded as sea levels rose to their present level. Part of the archaeological story necessary for meaningful correlation may no longer be available. The area Higham has chosen is linguistically diverse and there is an array of hypotheses conjoining the established language phyla in the region. After giving an overview of these hypotheses in relation to early agricultural expansions, he examines one such hypothesis in more detail. His choice is the Austric hypothesis, linking Austronesian and Austroasiatic. This is an old idea but one given a new impetus in studies by Blust (1996) and Reid (1994).

Evidence from Thailand in particular is used by Higham to dispute an earlier attempt by Meacham to link the archaeology of the region to language phylum distribution. On present evidence, Higham finds the recently revived Austric hypothesis attractive. The distribution of the proposed higher-order grouping is consonant with a history linked to the spread of rice agriculture from its early centre throughout the Southeast Asian region.

In relation to the further expansion of the Austronesian languages south through Island Southeast Asia and out into the Pacific, Spriggs (Ch. 4) takes up the story where Higham leaves off. Again the hypothesis is an explicit link between an expanding agricultural horizon and the spread of a major

language phylum. There is a long history in the Austronesian-speaking region of attempts to relate the remarkable spread of Austronesian languages more than halfway round the world from Madagascar to Easter Island to archaeological entities. Glossed broadly as the Island Southeast Asian Neolithic and its eastward extension, the Lapita Culture of Island Melanesia and Western Polynesia, an almost equally wide-ranging archaeological entity can be defined.

For areas such as Remote Oceania (Island Melanesia beyond the main Solomons chain, Polynesia and Micronesia) which appear to have been uninhabited before settlement by agricultural populations, the link between such populations and Austronesian languages is clear. This allows an absolute time-scale to be postulated with some confidence for the break-up of particular language sub-groupings within Remote Oceania. Once the dates of initial settlement or particular island groups are established (and this remains controversial for Eastern but not for Western Polynesia), then a powerful tool for looking at rates of change in the languages of the area is provided.

Such a tool is usually not available in the absence of written records of languages at different stages in their history. In world terms, the Pacific is very much a special case, reflecting the recent time-scale of human settlement in Remote Oceania. This probably unique direct correlation makes it much easier to establish earlier stages of agricultural and language expansion, by working back from the relatively simple culture history of Remote Oceania to the more complex situation in long-settled areas further west such as New Guinea and Island Southeast Asia.

The value of working back in this way is illustrated in Bellwood's chapter, which seeks to untangle a more complex history in an area of eastern Island Southeast Asia where both Austronesian and Non-Austronesian (Papuan) languages are spoken. Taking the established broad correlation of early neolithic culture and Austronesian expansion as his starting point, he presents a more detailed case study of a relatively small area. Bellwood makes the important methodological point that explanations for the distribution of language phyla are likely to be fundamentally different from explanations at the micro-scale of language distributions in particular bounded areas, such as individual islands.

Major differences in the dates of neolithic assemblages in the Halmahera area, 3500 BP in some places as opposed to 2000 BP elsewhere, correlate well with linguistic divisions. Detailed analyses of the languages themselves bring up some surprising evidence of comparatively recent language levelling among some Austronesian and Non-Austronesian groups compared with other 'relict' languages. The recent history of the Spice Islands from written as well as archaeological sources suggests plausible explanations for some of the linguistic complexities.

Ross (Ch. 6) approaches similar questions in relation to the Central Papuan coastal region of New Guinea starting from the linguistic rather than archaeological side. Noting that both archaeology and linguistics can produce independent sequences of 'events' which can then be compared, he constructs

a detailed linguistic event sequence for the Austronesian languages of Central Papua, involving evidence for initial colonization of the region from east to west, language fissure as communities became separated during the process of settlement and subsequent language contact with by now differentiated Austronesian-speaking areas further east.

There is a remarkable fit between the detailed linguistic sequence Ross presents (only partly sketched here) and the independently derived archaeological sequence from the area which shows initial pottery-using settlement from the east and a later cultural intrusion some thousand years later from the same direction. On archaeological grounds it has proved difficult to reach agreement on whether the current populations show cultural continuity from the earliest settlement period or represent populations which were part of the later intrusion. The linguistic evidence supports cultural continuity from the earlier period as the languages were not replaced by later contact with other linguistic communities but merely show contact-induced changes.

The detailed picture produced by micro-level comparison of linguistic and archaeological evidence differs from the regional presentations which precede the chapters by Bellwood and Ross. Bellwood's and Ross's detailed case studies are linked to the wider linguistic and archaeological picture. Bellwood (Ch. 5) considers how 'porous' linguistic boundaries can be at the local level, while for Ross they provide a context for his detailed examination of linguistic sequences within a language sub-group and to relate this to regional archaeological sequences. Bellwood's chapter in particular stresses the need for different types of explanation when considering phenomena at very different spatial scales. An entire phylum is unlikely to have arisen by mass language shift across a large area, or by an invading elite group forcing its language on tens of thousands of people. But at the local level such processes are entirely plausible explanations in particular cases.

On the surface, the final chapter in Part I by Evans and McConvell contradicts the model used in several of the other chapters, linking the origins and spread of agriculture and the distribution of large-scale phyla. Australia was the only continent inhabited solely by hunter-gatherers in the recent past, and yet the time-depth and pattern of spread of the Pama-Nyungan languages resemble the pattern elsewhere associated with expansion of agricultural groups. But there is a parallel with these explanations in that the expansion is seen as driven by technological innovations, not an agricultural technology but nevertheless allowing an intensification of food production: the introduction from Southeast Asia of the dingo, changes in stone tool technology, growth of trade networks, new vegetable foods and new food processing technologies, and subsequent ability to exploit marginal environments and underwrite population increase.

Evans and McConvell suggest that a model broader than the Renfrew–Bellwood one is needed to explain correlations between broad-scale linguistic and archaeological sequences. They note that Nichols's concept of a 'spread zone' (see her Ch. 10 in this volume) does not require agriculture but implies

other forms of socio-cultural change. Both in the Australian case, and also to provide an explanatory framework for pre-agricultural language distributions elsewhere in the world, a more encompassing model is probably required.

Archaeologists and linguists are generally in the business of promoting theories that have widespread applicability. The alternative, that there is in fact no one factor underlying the expansion of language phyla and that each case must be taken separately is not considered by any of the authors in this volume. In part, this is because the chaotic mosaics represented by Australian (as a whole, not Pama-Nyungan), Papuan and Amerind are not discussed. These 'geographical' phyla may lend themselves to particularist historical explanation of an unglamorous type.

Part II of this volume is 'Migration and expansion and their linguistic correlates: Eurasian case studies'. The earlier chapters have dealt with broad-scale distributions of particular phyla linked with Early to Mid-Holocene agricultural developments. Some chapters in this second section deal with an area of the Old World where multiple migrations and expansions in prehistoric and historic times have created even more complex patterns of language distribution. In some cases these have served to obscure an earlier pattern of agricultural expansion more like that discussed in the first section of the volume.

Janhunen (Ch. 8) covers Northeast Asia, defined as eastern Siberia, Mongolia, Manchuria, Korea and Japan. It is a linguistically complex region with ten well-established phyla, of which only two (Mongolic and Turkic) have members outside of this region. A further four phyla marginally touch upon the region. Northeast Asia has experienced many intrusions with expansions of language groups spreading from more central parts of the continent. In considering the homelands of the ten phyla based within the region, Janhunen wisely uses a linguistic method, the results of which can then be compared with the archaeological record.

What is striking in his analysis, especially when compared to historical and archaeological information, is the shallow time-depth for the apparent linguistic homogeneity of areas such as Korea, Japan and Mongol-speaking regions. For the latter, he suggests that the empire of Chinggis Khan in the period AD 1200–1400 erased earlier linguistic and ethnic diversity within Mongolic. The homogeneity of Korean and Japanese have arisen as a result of state-formation processes of the last two millennia and the elimination of the presumed relatives of these languages. For such an area models of linguistic spread associated with initial agricultural expansion are inadequate.

From Northeast Asia we move west to the adjacent area of North-Central Asia with Koryakova (Ch. 9). Many of the same issues of language and ethnic replacement are posed, but this time from a much more archaeologically based assessment. This produces an interesting contrast in style and focus of the two chapters. In Janhunen's chapter the languages are the focus, in Koryakova's the archaeological entities are foregrounded. 'Thick description' of the archaeological record is used in the Russian archaeological tradition

to trace detailed links between languages, archaeological cultures and ethnic groups through to their recent histories. Koryakova's chapter gives an insight into the methodology of the Russian tradition and adumbrates various guiding models used in consideration of this dynamic zone where ideas, material cultural complexes and languages have interacted over the last 4,000 years.

The broad sweep of Indo-European history is also covered by Nichols (Ch. 10), in an area overlapping with that covered by Koryakova. She views the Central Eurasian grasslands as a spread zone: 'a region where a single language family spreads out widely, genetic diversity of languages is low, and one language family replaces another over most of the area every few millennia' (pp. 220–1). Spread zones can be expected at high latitudes and in dry and/or seasonal continental interiors where population density would be low. The chapter seeks to develop a theory for describing linguistic spreads in linguistic and linguistic-geographical terms. In doing so, Nichols deconstructs the 'uniqueness' of the pattern of Indo-European spread, positing instead generic events to account for it, and the subsequent spreads in the same area of Iranian, Turkic and Mongol languages in later periods. She suggests the directionality of spread is long-standing and may even extend to pre-neolithic periods.

Nichols challenges many commonly held but perhaps undertheorized beliefs about languages. She suggests that language spreads are not mainly the results of migration, but involve a substantial amount of language shift. In spread zones at least languages tend to move against the direction of major economic innovations such as agriculture and stockbreeding. Ideas central to several of the earlier presentations in this volume are thus challenged. Nichols also posits that genetic diversity will accumulate not near the origin of a language spread but at its periphery; a further idea for linguists and archaeologists to think on. In its conclusions, the chapter represents a major challenge to almost all scholarly factions concerned with Indo-European origins and spread. Her suggestion that the first and possibly even the second of Gimbutas' Kurgan expansions took place before the spread of Indo-European and thus must represent earlier languages moving across the spread zone is particularly provocative for archaeologists.

Krell (Ch. 11) takes up the critique of Gimbutas' Early Kurgan expansion as representing Proto-Indo-European culture, showing that the fit between the subsistence economy found on Kurgan sites and that reconstructible to Proto-Indo-European is by no means as exact as Gimbutas claimed. Read on its own or in the light of Nichols' preceding critique, it represents an important challenge from the linguistic side to what has been until recently one of the more widely accepted attempts to relate archaeological and linguistic sequences in the Eurasian region.

Finally, Hines (Ch. 12) moves the discussion further west and closer in time to consider the origins of English. The micro-level of analysis after regional expositions stands in the same relation to this section as the chapters of Bellwood and Ross did to the previous one. We are reminded that different

explanations are required at different spatial scales and that different explanations may be required in different socio-political situations.

Nichols had already alerted us to this latter point when discussing the reversal of the usual east–west trajectory of language spread in the steppe zone with the historical expansion of the Russian empire and language in the last few hundred years. Hines raises it again with his example of language divergence within a period of more intense communication among Germanic-speaking groups as political groupings became stronger and sought to mark their territories by asserting differences, including differences in language. The rapid 'creation' of English out of what must have been somewhat divergent dialects of different groups of settlers moved faster than the standardization of the somewhat divergent material culture of these same groups as a shared Germanic/English identity was created from that of Angles, Saxons and others. The others presumably included the Romanized Celts already in occupation of the land, but there is interestingly no linguistic influence from that source on the form of English which was created. At the local level, language shift is again seen as significant as people moved towards prestigious target languages or forms of languages, in England as much as in Halmahera.

Although Hines's study is at the local level he, like Nichols, is also concerned to raise issues that may challenge linguistic models of wider-level phenomena. In this case it is the place of assimilation and convergence in producing the formation of branches within families much later than conventional reconstructions would suggest.

Part III of this volume is concerned with 'Linguistic models in reconstructing subsistence systems'. Krell has already raised this issue as part of a wider critique of Gimbutas's reconstruction of the linguistic homeland of Proto-Indo-European and its suggested archaeological correlates. In this section a geographically and methodologically wide-ranging collection of chapters looks at linguistic evidence as providing clues to the origins and diffusion of particular crops and agricultural technologies between language groups.

The case of Mixe-Zoquean languages discussed by Wichmann (Ch. 13) is particularly striking for two reasons; the claim that Mixe-Zoquean is an example of agricultural expansion because of the significance of reconstructed food-plants, and the use of the reconstructed proto-language to interpret Epi-Olmec. Wichmann shows that the movement of food plants and domestic animals, even those indigenous to the region, remains complex with considerable cross-borrowing both between branches and with other language groups. This constitutes a warning concerning simple assumptions about the reconstruction of agriculture based on limited datasets and faulty phonology. Wichmann, through an improved reconstruction of Proto-Mixe-Zoquean, is also able to make more convincing suggestions for the interpretation of Olmec glyphs. This example may be unique in that a proto-language is used for epigraphy instead of parallel texts in contemporary languages.

Connell (Ch. 14) also consider a similar case in Southeast Nigeria among speakers of Cross River languages. Although it has generally been assumed that

yams and palms (especially the oil palm) are part of the early vegecultural systems developed in the tropical forest of West Africa, no support has been forthcoming for this hypothesis either from archaeology or linguistics. These plants have the additional problem that they are part of the native flora of West Africa and thus even non-agricultural populations are likely to have exploited them and have had pre-existing terminology to describe them. By compiling the evidence in some detail, Connell is able to show, as in the Mixe-Zoque case, there is a remarkable amount of local borrowing, even in names for indigenous plants. By examining not only the names of the plants but also terms associated with cultivation, he is able to show that these associated terms do not reconstruct to proto-Cross River but only within some of its branches. This suggests strongly that the speakers of the proto-language were exploiting only the wild plants but that when the language split into sub-groups, agricultural technologies emerged. This case study is an example of what can be achieved by combining linguistics and basic botany; the lacuna is of course, absolute dates, which can only be supplied by archaeology.

The next two chapters concern the spread of rice cultivation in East and Southeast Asia. Vovin (Ch. 15) uses rice terminology to look at the linguistic affiliation of the rice-using Yayoi culture of Japan (third century BC to third century AD), often seen as the basis of historical Japanese culture. He challenges this view, suggesting instead an Austroasiatic affiliation for the Yayoi culture, overwhelmed by Kofun culture invaders from Korea in the fourth century AD. According to Vovin, the reconstruction of rice terminology shows this Austroasiatic substrate overlain by an Altaic terminology adopted from Korean settlers as near-complete language shift occurred. This linguistic analysis fits with the view of several Japanese archaeologists that the Yayoi culture came from coastal Southeast China, where Austroasiatic languages would have been spoken at the time. A further clue is provided by the ethnic name of the Yayoi in early historical sources, Wa, an ethnonym common among present-day Austroasiatic speaking groups in Yunnan, Burma and Thailand.

Pejros and Shnirelman, a linguist and an archaeologist, have collaborated in Chapter 16 on the questions of the origins and spread of rice in Southeast Asia. Pejros has argued in print for a different version of Austric from that discussed earlier, including Miao-Yao and Daic, and he links this to the history of rice agriculture in the region, situating one area of origin of domesticated rice among Sino-Tibetan speakers as does van Driem. The authors focus on the two species of domestic rice, *Oryza indica* and *Oryza japonica*, which presumably had different centres of domestication and were moved around independently in the early period of rice cultivation. Pejros's and Shnirelman's concern is with its transmission ultimately to Austronesian-speaking groups as they moved out into Island Southeast Asia and also its movement to further India where the evidence is that it was borrowed by the Dravidians from Sino-Tibetan speakers.

The final chapter by Mahdi develops the theme of the spread of cultigens in the Southeast Asia region, extending the purview to South Asia as a recipient of

these cultigens by trade and perhaps direct Austronesian settlement in India and Sri Lanka in a period later than that considered by the other contributors. As with Wichmann and Connell, Mahdi's reconstructions suggest a complex flow of useful plants between India and Southeast Asia with loans going in both directions. His many controversial conclusions, based on his reading of linguistic, mythical and historical sources need to be read in conjunction with the related chapters in Volume III.

As was true of van Driem, Mahdi has to rely on archaeological sources which are either scanty or belong to a 'primitive' phase of the discipline. Much more archaeological research needs to be undertaken in various areas critical to Mahdi's hypotheses. As seems common in the wider East and South Asian region, linguistic research is at a more mature stage of development than archaeology. The differing rates of development of the two disciplines are a definite barrier to meaningful comparison.

The chapters in this volume, while not shying away from theoretical and methodological discussion, have been mainly concerned with presenting case studies attempting to correlate archaeological and linguistic hypotheses. As Ross points out, linguists and archaeologists both construct sequences of events, the one linguistic and the other material-cultural. The data they rely on are, however, very different in kind, as are the methodologies involved in processing those data to produce the sequences in question. At least in theory, it is only once their particular methodologies have been applied and independent sequences constructed that correlation should be attempted. However, especially where archaeological data are too exiguous to generate the required sequence, bolder spirits are likely to venture on to more speculative ground. Indeed, as Ross reminds us, it would be irrational not to attempt such correlation; both kinds of reconstructed events are manifestations of important social changes in the human societies studied by the two disciplines.

NOTE

1 This is confusing to all but specialists in this region. Tibeto-Burman is generally considered a sub-group of Sino-Tibetan. However, if van Driem is correct and Chinese is in fact part of Sino-Bodic then it is more appropriate to call the entire phylum Tibeto-Burman.

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Part I

CORRELATING ARCHAEOLOGICAL AND LINGUISTIC SEQUENCES

1 *Archaeology, language and the peopling of West Africa: a consideration of the evidence*

KEVIN C. MACDONALD

> Over the past two decades, archaeology has looked with considerable disfavour on the work of those earlier generations of scholars who sought to explain the changes observed to have taken place in the archaeological record in terms mainly of migrations. We now see that the particular kinds of pottery so meticulously studied in the past are not necessarily secure indicators of particular groups of people – the pots themselves could have been traded, or a fashion of pot-making adopted, without any change in population. We see more clearly that social groups are not necessarily precisely the same thing as linguistic groups, and are much more willing to accept changes in the archaeological record as the result of locally-occurring developments. . . . But have we thrown the baby out with the bathwater?
>
> (Renfrew 1989: 3)

INTRODUCTION

Since the 1950s there has been a general tendency in archaeology to eschew the drawing of broad black arrows across continental maps: migration theories are out of fashion. Yet in Africa, the use of such broad black arrows to indicate the movement of monolithic 'cultures', 'peoples' or 'language groups' has continued almost unabated to the present day. In northern and western Africa two mobile monoliths, the African Aqualithic (Sutton 1974, 1977) and the '*Néolithique de Tradition Soudanais* (or *Saharo-Soudanais*)' (Camps 1972, 1974), spring immediately to mind. Contradictorily, both have been used by historical linguists and archaeologists as a peg on which to hang the Nilo-Saharan linguistic phylum (Sutton 1974; Haaland 1991; Ehret 1993). Such 'arrow drawing', has long had a detrimental effect on the rigour of archaeological and linguistic research in the Sahara and the Sahel. We have taken shelter in generalities, and not sought complexity or diversity. Certainly, I

would agree with two eloquent summations of the subject made by Maitre in the 1970s:

> Certain chercheurs en chambre, attirés par l'immensité saharienne, se sont empressés de jongler avec les tessons et fragments de harpons, traçant de grandes flèches du Nil en Mauritanie, du Niger au Nord-Sahara pour nous démontrer quelles furent les routes d'invasion des hommes d'Esh-Shaheinab, porteurs du harpon et du vase décoré à la wavy-line. . . . Il est inutile de souligner combien cette conception d'une Préhistoire soumise aux conquetes et aux luttes d'influences relève plus de l'Ecole de Guerre que de la recherche scientifique.[1]
>
> (Maitre 1972: 125)

> Le 'Néolithique saharo-sudanais' n'aurait sans doute jamais vu le jour si l'on avait fait preuve de prudence et de pondération, et si l'on s'était d'abord attaché à l'étude des particularismes régionaux plutot qu'aux vastes synthèses.[2]
>
> (Maitre 1979: 601)

Regionally based archaeologies, studying the finer details of stylistic expressions of group identity, socio-economic interactions between distinct entities, as well as local responses to environmental change, are only beginning to appear, and are in fact long overdue. Yet as this proceeds, I hope we shall not, in the words of Renfrew, 'throw the baby out with the bathwater' and abandon, as archaeologists, the terrain of 'language and archaeology' to historical linguists. Indeed a closer and more explicit collaboration between the two disciplines is required to produce a richer view of the past, and to avoid circularities in argument (with linguists cribbing the work of archaeologists to buttress or lend absolute chronology to their theories, and archaeologists citing the linguistic models so formulated in order to support their own). Certainly a more critical and complex perspective is required if we are reasonably to come to grips with what was surely a labyrinthine phenomenon: the peopling of West Africa.

The current occupants of West Africa represent the Niger-Congo, Nilo-Saharan and Afroasiatic language phyla. Of these the overwhelming majority of modern languages belong to the diverse Niger-Congo family (including those spoken by the Fulani (Peulh), the Mande peoples, the Mossi, the Senufo, etc.). Fewer groups, albeit significant ones, speak Nilo-Saharan (e.g. the Songhai) or Afroasiatic languages (e.g. the Tamelsheq and Moor).

How such a profusion of groups came to be living in proximity to each other has long been a matter for academic speculation. In 1985, linguist Thomas Hale and anthropologist Paul Stoller, coined the term 'Deep Culture' to describe the myriad ritual, economic and social links existing between the ethnic groups of the West African Sahel. The word 'deep' here implies the unknown time-depth which has allowed the accretion of so many cultural

themes among Middle Niger ethnicities that they begin to resemble a single, symbiotic cultural organism.

In this chapter the formation of Sahelian 'Deep Culture' is considered at three different time-depths: the Terminal Pleistocene and Early Holocene (12,000–8000 BP), the Mid-Holocene (8000–4000 BP), and the Recent Holocene (4000–2000 BP).[3] In the most recent period, the Middle Niger is focused upon so as to shed light on the complications induced by the compaction of human territories at the end of the Holocene pluvial (wet phase). These synthetic essays, focusing at the level of broad cultural traditions, will demonstrate that the current linguistic hypotheses regarding the situation and dispersal of African proto-languages (e.g. Ehret 1993) are in danger of grave error due to their implicit connection with outmoded archaeological models. Still, though some parallels may be drawn between the spread of African language phyla and Renfrew's (1989) proposed solution to the Indo-European debate, the very nature of West African climatic history necessitates a greater consideration of prehistoric population movements than is currently the fashion. In constructing models for the historical progress of language families in West Africa, the phenomena of language 'over-printing' must be considered: both at the level of total language extinction (e.g. Pygmy languages eclipsed by Bantu; cf. Vansina 1990) and at the level of more localized language replacements (e.g. the expansion of Hausa over widely divergent neighbouring languages; cf. Sommer 1992). One thing is clear – historical linguistics does not possess sufficient pieces on its own to reconstruct the puzzle of West African population origins.

ARCHAEOLOGICAL EVIDENCE FOR THE EARLY HOLOCENE PEOPLING OF WEST AFRICA

Around 12,000 BP, the Sahara, which had lain an uninhabitable waste for more than 8,000 years due to the ravages of the Late Pleistocene hyper-arid, began to receive the agreeable effects of post-glacial climatic amelioration: rain fell, waterbodies grew, soils formed, vegetation returned. It is into this suddenly fertile void that prehistorians have long flung peoples from the Nile Valley, be they Aqualithic fisherfolk or Shaheinab Neolithic agro-pastoralists. Undeniably we must speak of migrations when we consider the Holocene prehistory of the Sahara and its fringes, for much of what is now Sahel and Savanna in West Africa was virtually uninhabitable between 20,000 and 12,000 BP (cf. Petit-Maire and Riser 1983; Grove 1985). However, the comparative abundance of archaeological studies in the vicinity of the Nile Valley have too long privileged that area as a source for Holocene Saharan migrants, and thus proto-languages. Indeed other refugia, including the Maghreb, Cyrenaica, and the West African Forest, must be considered if we are to reach an accurate understanding of current ethnic and linguistic diversity.

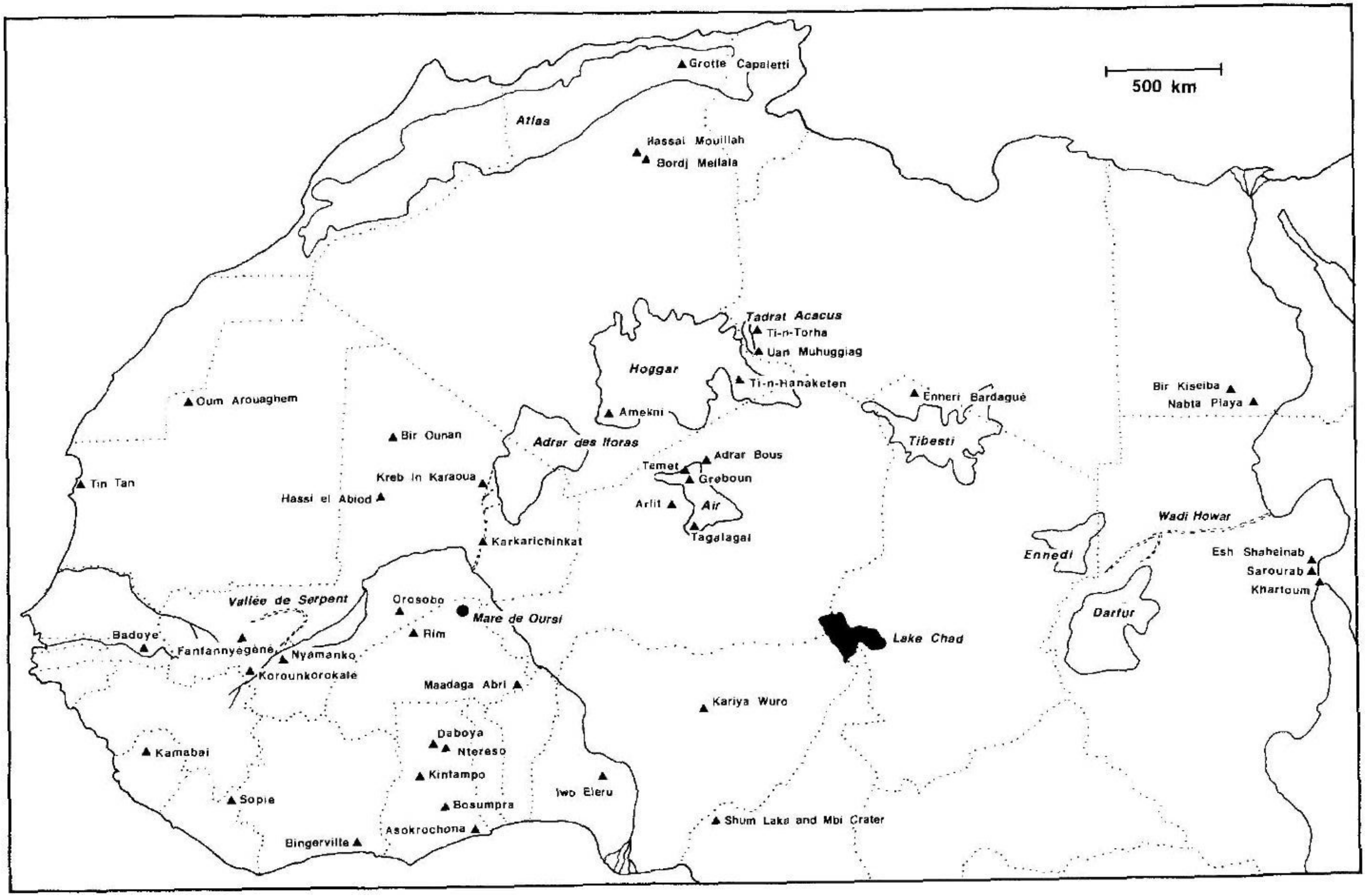

Figure 1.1 Locations of Terminal Pleistocene and Early to Mid-Holocene sites mentioned in the text

Peopling from the north and east (12,000–8,000 BP)

For the Terminal Pleistocene, there exist consistent – if sparse – indications of a pre-ceramic 'Upper' or 'Epi-Palaeolithic' in the Sahara from the beginning of the post-glacial wet phase. Most of these occurrences have previously been classed under the overarching sobriquet of 'Ounanian' by J.D. Clark (1980), who believes them to date to between 10,000 and 8,000 BP. The Ounanian is characterized by the presence of shouldered and/or tanged points made on blades ('Ounanian points'), which are accompanied by technologically variable lithic industries (usually blade-based, but sometimes bladelet-based, with or without ground stone artefacts, etc.) (Breuil 1930; Clark *et al.* 1973; Camps 1974; Clark 1980). The primary distribution area of these Ounanian sites falls within the Saharan climes of Mauritania, Mali, Algeria and Niger (the undated type site, Bir Ounan, is in the Taoudenni Basin of Mali: Breuil 1930) (Figure 1.1).

Findings by Roset (1987) in Niger have called into question the aceramic 'Epi-Palaeolithic' nature of the Ounanian. At Temet, near the edge of the Air Massif, Roset found a composite industry of geometric microliths, Ounanian points, and blade-based tools in sealed lacustrine deposits dating to between 9550±100 and 8565±100 BP. Although no ceramics were found in the deposits, a stone 'potters comb' and a 'stone bowl' were recovered. However, at the nearby fossil dune-top site of Adrar Bous 10, a suite of lithic artefacts identical to that of Temet was found associated with quantities of grinding stones and ceramics, dated on associated charcoal to 9130±65 BP. Critically, these sites mixed two discrete industries encountered by Clark *et al.* (1973) at different sites in the same region: the Ounanian and the Kiffian (a geometric microlithic ceramic industry associated with bone harpoons, post-dating 7310±120 BP). Smith (1993a) has defended a discrete aceramic, and non-microlithic, 'Epi-Palaeolithic' status of the Ounanian by invoking a process of deposition-deflation-deposition to account for the inter-mixing of these artefacts in Roset's samples. Indeed, it is interesting to note that Tin-Ouaffadene, a third 'mixed-industry' located near Adrar Bous, has groundstone but lacks any indication of ceramics (dates 9220±140, 9080±230, and 9060±240 BP: Roset 1987: 223); and that Tagalagal, a small sediment trap with the earliest ceramics yet dated in the Sahara (dates 9370±130, 9330±130 BP: Roset 1987: 218) possesses a crude flake-based industry with bifacial projectile points absolutely unlike 'Ounanian' and 'Kiffian' assemblages. Thus, Roset's case for both long-term cultural homogeneity and the absence of an Epi-Palaeolithic in the Adrar Bous region remain far from proven.

Unfortunately, pure aceramic 'Epi-Palaeolithic' sites other than those cited above remain widely distributed and poorly dated (e.g. Greboun Wadi [Niger]: Clark *et al.* 1973; Kreb In Karaoua [Mali]: Gaussen and Gaussen 1988; Bordj Mellala and El Hadjar [Algeria]: Aumassip 1986; and Oum Arouaghem [Mauritania]: Vernet 1993). However, in certain cases, a continuity with more recent ceramic-using traditions has been demonstrated, such as at the open-air

site of Hassai Mouillah in the Algerian Hoggar, where an aceramic microlithic industry dated to 8600±150 BP underlies a ceramic-bearing, 'Neolithic' horizon (Marmier and Trécolle 1968; Aumassip 1986). Additionally, in the Wadi Ti-n-Torha complex of the Libyan Tadrat Acacus, a continuum of aceramic 'Epi-Palaeolithic' to ceramic 'Neolithic' has been documented on a regional scale, with an aceramic 'Epi-Palaeolithic' date of 9080±70 BP at Wadi Ti-n-Torha East Shelter, underlying ceramic associated deposits dated to 8640±70 BP (Barich 1987: 102).

Raimbault (1990) has utilized the term Ounanian to denote one of the 'Neolithic' facies of the Malian Sahara. Admitting that more 'precocious' pre-ceramic occurrences probably exist below the shores of the Taoudenni's palaeolakes, he asserts a post-8000 BP survival of the Ounanian tradition (*c.* 7000 to 6000 BP). At seventeen sites in the vicinity of the Erg Atouila and Bir Ounan, an Ounanian blade and point industry (typologically close to that of the Algerian 'Epi-Palaeolithic') is associated with grinding stones and two discrete ceramic traditions (Raimbault 1990: 68–70). These data are of particular interest since they may demonstrate the continuance and diversification of the early Holocene Ounanian tradition, already identified as a discrete pre-ceramic industry in the same region (Raimbault 1983).

The most important lesson to be drawn from this evidence is the following: before the putative 'Aqualithic fisherfolk' expanded from the Upper Nile into the Sahara (cf. Sutton 1974, 1977; Haaland 1993), the region was already populated by a broad swathe of diverse hunting-gathering peoples who show in some cases a direct lithic continuity with later ceramic-using traditions. Additionally, if we accept any of Roset's (1987) pre-9000 BP dates for ceramics in the Nigerian Sahara (i.e. from Tagalagal and Adrar Bous 10), then any implied east to west directionality in this technology's spread must be in doubt.[4]

J.D. Clark (1980: 562–5) proposed that Saharan Epi-Palaeolithic groups were hunter-gatherers with an economic adaptation to arid environments which allowed them to be on the cutting edge of the Sahara's recolonization. Their traces are thus ephemeral, and often obscured by the more substantive accumulations of later post-Pleistocene bone and shell middens. From where could these Terminal Pleistocene hunter-gatherer populations have derived?

Individual assemblages have, on typological grounds, been connected to industries as far flung as the Shamarkian of Nubia (cf. Close 1987, on the Wadi Ti-N-Torha tradition), the 'Libyan Culture' of the Western Desert of Egypt (cf. Clark 1980: 563) and the Iberomaurusian/Eastern Oranian of the Maghreb and Cyrenaica (Smith 1993a). Hays's (1974, 1976) preliminary work comparing early Holocene lithic assemblages from the central Sahara and the Upper Nile certainly showed little parallel between them. However, such essentially qualitative rather than reliably quantitative comparisons are, in our present state of knowledge, highly tenuous. Indeed, Aumassip (1986) has probably taken the right track by baptizing a localized Epi-Palaeolithic industry

of southern Algeria with its own name: the Mellalaian. This industry, characterized by high quantities of backed bladelets, stands independent of its Maghrebian contemporaries (the Capsian and Iberomaurusian) and encompasses sites from the Ouargla basin such as Hassi Mouillah, Bordj Mellala and El Hadjar.

It is likely that the Mellalian, the Ounanian and some other Saharan 'Epi-Palaeolithic' industries derive from the post-Aterian 'Horizon Collignon' – a poorly chronologically defined, backed-blade industry known from the Maghreb (cf. Camps 1974: Fig. 59). It is also possible that the Horizon Collignon may also be ancestral to the Iberomaurusian (*c.* 15,000–9000 BP) (ibid.). Indeed, if one accepts the rather recent *c.* 20,000 BP denouement for the Aterian put forward by Tillet (1985, 1987), then it is also possible to suppose a common ancestry for the Iberomaurusian, Mellalian and Ounanian with the Aterian (via the Horizon Collignon), with the latter two being the more inland and arid-adapted aspects of this trio. Such an assumption is of particular interest if we consider the Terminal Pleistocene Saharan repopulation hypothesis of the physical anthropologist Olivier Dutour (1989). Dutour has followed the trail of a Terminal Pleistocene and early Holocene robust physical type known as 'Mechtoids' around northern Africa, and has demonstrated similar population elements in the Maghreb (the Iberomaurusian), Nubia (Wadi Halfa, Djebel Sahab, etc.), and in the Sahara (Hassi el Abiod, Tin Tan) (Dutour and Petit Maire 1983; Dutour 1989). Dutour (1989: 273–8), however, does not propose an initial repopulation from the Nile Valley, but from the northern margins of the Terminal Pleistocene Sahara where Aterian refuge populations may have survived and developed in highland regions between 20,000 and 12,000 BP. While such a proposal goes against the grain of long-held assumptions concerning the life-sustaining ability of Saharan environments during the Late Pleistocene hyper-arid, it does have the virtue of parsimoniously explaining the uniqueness of the sparse Saharan Epi-Palaeolithic industries and the subsequently wide distributions of Mechtoid populations. Whatever way we choose to finesse this population hypothesis, however, the essential factor to bear in mind is that we need not evoke 'Aqualithic' (or in French parlance 'Néolithique Ancien' of the 'Néolithique de Tradition Saharo-Sudanaise') populations to expand with their ceramics into a Saharan void. Instead we may be in the presence of a 'bow wave' diffusion of ceramic technology around 9500 BP, the directionality of which is still not clear.

Peopling from the south (12,000–8000 BP)

A surprisingly neglected aspect in the peopling of West Africa and the Sahara is the importance of an explicitly Sub-Saharan input. Sub-Saharan populations lived out the Ogolian hyper-arid (*c.* 20,000–12,000 BP) in coastal refugia, and in areas which are now part of the West-Central forest zone. Indeed, from Shum Laka rockshelter and Mbi Crater in Cameroun we now have occupational sequences spanning at least the last 30,000 and 12,000 years respectively (de Maret *et al.* 1987, 1995; Asombang 1988). Yet as we move

to the west, our earliest dates appear to be more recent. Potentially this may indicate a retreat of populations into the eastern corner of West Africa during the peak hyper-arid, or may simply reflect the small amount of late stone age research in this region.

In Nigeria, the earliest known occupation during the Terminal Pleistocene is still that of the basal layers at Iwo Eleru rockshelter, where human remains and a non-geometric microlithic industry have been dated to as early as 11,200±200 BP (Shaw and Daniels 1984). In southern Ghana, at the open-air sites of Temet West and Asokrochona, sand formations dating to the Ogolian hyper-arid (*c.* 18,000–13,000 BP) have been shown to contain rare traces of prepared core industries (Nygaard and Talbot 1984). Finally, the Bingerville Highway site in southern Ivory Coast has provided a non-geometric microlithic industry dated on associated charcoal to 13,050±230 BP (Chenorkian 1983). Thus, it would appear that by at least *c.* 12,000 BP the West African littoral was occupied by scattered groups of hunter-gatherer peoples.

At some point after 12,000 BP these indigenous populations began an expansion northwards which is attested to by an increasing density of non-geometric and geometric quartz microlithic industries from the coast to as far north as Mali before 8000 BP (Korounkorokalé: MacDonald 1994) and Burkina Faso (Rim: Andah 1978; Maadaga Abri: Breunig and Wotzka 1991). Additionally, there appears to have been some persistence or reinvention of prepared core flake industries during the Early Holocene of the modern Sahel zone (cf. MacDonald and Allsworth-Jones 1994). These industries, whose primary tool types consist of large scrapers, are known from imprecisely dated Late Pleistocene and Early Holocene contexts in the Sahel from Senegal to Niger (Badoye: Camara and Dubosq 1987; Vallée de Serpent sites: MacDonald and Allsworth-Jones 1994; Orosobo: Gallay *et al.* 1995; Mekrou: Vernet 1994). The point of origin for these macrolithic occurrences is difficult to fix at present: they may simply have been regional adaptations of microlith fabricating peoples denied all but the coarsest of raw material types (e.g. prepared core/Levallois flake based tools form the heavy duty component of the Shum Laka microlithic industry, cf. de Maret *et al.* 1987), or they may represent a second discrete Sub-Saharan tradition.

ARCHAEOLOGICAL EVIDENCE FOR THE MID-HOLOCENE PEOPLING OF WEST AFRICA

By as early as the eighth millennium BP, it is possible that pastoralists and their domestic cattle had entered the Sahara. Despite this new economic adaptation, which was well dispersed by 5500 BP, specialized fishing peoples and generalized hunter-gatherers were to persist in West Africa throughout the Mid-Holocene. Tantalizingly, it is during this period of pastoral expansion that we have many self-depictions by the region's inhabitants in the Saharan

rock art corpus. From such depictions it is well-evidenced that we are not dealing with ethnic homogeneity within Mid-Holocene pastoral societies (see pp. 44–7).

Pastoral peoples of the Sahara and its margins (8000–4000 BP)

Scattered throughout the Central Sahara are stone piles and circles thought to be the hearths and windbreak foundations of vanished nomads. Gabriel (1973, 1987) after undertaking years of systematic research on these *steinplatze* has been able to establish an impressive group of radiocarbon determinations placing their probable age between 8500 and 3800 BP, with their greatest temporal concentration between 5800 and 5000 BP. At one of these sites, known as Enneri Bardagué, cattle remains have been identified in contexts dated to between 7635 and 5060 BP on three C14 determinations (Gautier 1987a). If Gabriel's *steinplatze* were indeed inhabited by pastoralists by *c.* 8500–7500 BP then weight would be added to Wendorf and Schild's (1994) argument for indigenous African cattle domestication in the Eastern Sahara between 9500 and 8000 BP. If not, then one must place the initial penetration of the Central Sahara by pastoralists between 7000 and 6000 BP, when more substantial and well-documented faunal assemblages bearing both cattle and ovicaprine remains are known from Ti-n-Torha North and Uan Muhuggiag in the Tadrat Acacus (Gautier 1987b), and from Ti-n-Hanaketen in the Tassili-n-Ajjer (Aumassip 1980–1; Aumassip and Delibrias 1982–3). In any event, it is likely that these early pastoral populations derived from northeastern Africa where domestic stock would have been available either indigenously or as a Levantine import.

From 5500 to 4000 BP pastoralists were active throughout the present-day Sahara (Gautier 1987a). The geographical origins of these pastoral populations are not instantly apparent, being obscured by overlapping swathes of bow-wave cultural diffusion. However it would appear that in at least two instances important founding populations entered arid West Africa from the north and the east.

Evidence for movement from the Upper Nile into the Sahara during the fifth millennium BP comes from the Wadi Howar in northwestern Sudan (Keding 1993; Kroeplin 1993). There, numerous cattle remains (dated to 5350±275 and 3915±210 BP: Kroeplin 1993: 255) are associated with ceramic assemblages having strong decorative motif affinities with the 'Khartoum Neolithic'. A decorative technique based approach to the region's ceramics has indicated a strong chain of affinities stretching from the upper Nile, through the Wadi Howar to the Lake Chad basin, with more tentative connections extending to the Malian Sahara (Keding 1993). Dubbing this tradition the '*Leiterband* complex' (*Leiterband* being a prominent pottery décor type), Keding (ibid. and pers. comm.) has posited its local expansion from the vicinity of Esh Shaheinab during the fifth millennium BP, with an increase in mobility carrying it to the vicinity of Lake Chad and beyond around 4000 BP.

It is interesting to note here that no earlier pastoral migrations from the Upper Nile to Lake Chad have yet been documented. Smith (1980) attempted to place the pastoral 'Tenerian Neolithic' of Niger within a 'Khartoum Neolithic' area of influence on the basis of a single, tenuous artefactual correlation (i.e. the presence of flaked stone adzes similar to those known from Esh Shaheinab). Other comparisons put forward on the basis of ceramics were not systematic, and direct comparison of published illustrations (Arkell 1953; Smith 1980) show very different decorative tools at work. The Tenerian, which has been dated from Adrar Bous and sites around Arlit to between *c.* 5700 and 4400 BP (Smith 1980; Gautier 1987a), may just as easily be viewed as the result of a localized development from Central Saharan pastoral cultures known to have existed from *c.* 7000 BP.

As to the north, we know that domestic cattle were present in the Maghreb by 6500 BP (Grotte Capaletti: Roubet 1979). The Iberomaurusian had been effectively displaced or subsumed by the Capsian throughout much of the Maghreb from 9000 BP, but distinct Iberomaurusian descendants did remain (e.g. the Cloumnatien) (Camps 1974). The Capsian, a microlithic industrial complex with postulated Levantine origins (cf. Camps-Fabrer 1989), widely obtained ceramic technology and cattle around 6000 BP becoming the 'Néolithique de Tradition Capsienne' (Camps 1974; Roubet 1979). As a material cultural entity, its presence never extended south of the Saharan highlands, although it is likely that population elements may have penetrated farther to the south after *c.* 4000 BP.

Unfortunately for those who have anticipated an early agriculture throughout the Sahelo-Sudanic belt, domesticated cereals appear from current evidence to be absent in West Africa until after 4000 BP. Studies of pollen cores from the Mare d'Oursi in Burkina Faso, and the study of wood charcoal from archaeological sites near Lake Chad in Nigeria, have indicated that agricultural land clearance only began after 3000 BP in these regions (Ballouche and Neumann 1995). Elsewhere, we are left only with our early dates for millet impressions on pottery from the many sites of Dhar Tichitt-Oualata in Mauritania (*c.* 3500–2750 BP: Munson 1976; Amblard and Pernès 1989). Although these unpromising results may be only local, it appears that pastoralism – coupled with extensive grain *gathering* – remained the dominant agricultural economy in West Africa until the Recent Holocene.

Aquatic adaptations (8000–4000 BP)

The more the entire notion of the 'Aqualithic' (Sutton 1974, 1977) is scrutinized, the more it vanishes before our eyes. We are now unsure about an east to west directionality for the spread of ceramic technology, a former Aqualithic hallmark. The 'wavy-line' pottery motif which was once used to distinguish it has been shown to have been originally poorly defined, often confused with other techniques in analyses and, further, to be very rare outside of the Nile Valley (Maitre 1972, 1979; Hays 1974; Caneva and Marks 1990). Such true Saharan 'wavy-line' finds as there are, occur only as scattered sherds

in vast assemblages, are ill provenanced and undated, or are dated to several millennia after the end of the Khartoum Mesolithic (Caneva and Marks 1990: 22). In contrast, 'dotted wavy-line' pottery motifs would now appear to have originated in the Sahara and spread to the Nile Valley through exogamy, or exchange and borrowing (Caneva 1993). Still, there are the bone harpoons, and the entire aquatic subsistence adaptation which first appears in the Late Pleistocene of East and Central Africa's Lakes Region (e.g. Ishango: Brooks and Smith 1987; Lowasera: Phillipson 1977a). But even harpoon data, when scrutinized, reveal some odd inconsistencies. From all of the early ceramic sites of central Niger bone harpoons and points are absent (Roset 1987). From Chad, the main connecting point in any Aqualithic diaspora, only a single 'neolithic' harpoon is known (from Ounianga Kebir) with the eleven other find spots usually illustrated on Aqualithic distribution maps pertaining to Iron Age finds (Treinen-Claustre 1982; Choi 1989). Besides, may we really infer cultural relations on the basis of a single functional item whose presence in Africa is now known to stretch into the Late Pleistocene?

Indeed, we are at a disadvantage in tracking even a limited *c.* 9000–7000 BP expansion of an Aquatic adaptation. The few well-researched sites which would correspond all belong to the Hassi el Abiod and Tin Tan facies of the Malian Sahara and the Mauritanian littoral respectively (Petit-Maire 1979; Petit-Maire and Riser 1983; Raimbault 1990). These facies, which date from the seventh millennium BP and continue into the Recent Holocene, are of non-food-producing, primarily fishing peoples, equipped with geometric microlithic industries and globular spatula and comb marked pottery, but only in the case of Hassi el Abiod are bone harpoons present. No true 'wavy-line' pottery (*sensu* Caneva) has been identified from these sites, and there are no striking parallels between the Tin Tan and Hassi el Abiod material cultures. Thus, while an Aquatic adaptation may have spread as an idea from the Nile, or even the Chad Basin during the Early to Mid-Holocene, the cultural unity of such an adaptation must remain strongly in doubt.

The persistence of hunter-gatherer populations in the south (8000–2000 BP)

Along the great river basins of the south, and in the coastal forests, the descendants of groups installed in the Ogolian coastal refugia remained widely distributed during the Mid-Holocene. Microlithic industries resembling those with which they are associated, however, are virtually absent north of the modern Savanna/Sahel ecotone. It is possible that this demarcation may have formed some sort of ecological barrier to their primary subsistence practices. Between 5500 and 4000 BP ceramic technology became rapidly widespread among these groups, with ceramic-bearing layers dated to 5370±100 BP at Bosumpra Cave, Ghana (Smith 1975), 5180±90 BP at Korounkorokalé, Mali (MacDonald 1994) and 4180±160 BP at Mbi Crater, Cameroun (Asombang 1988). Coupled with the advent of ceramics are some uncertain signs of economic intensification, particularly the exploitation of oil palm and possibly tubers (Stahl 1993). Still, from the ephemeral nature of known cultural deposits

and their sparse distribution it is likely that these societies remained highly mobile during the Mid-Holocene and continued to practise a primarily hunter-gatherer way of life.

Between 4000 and 3500 BP dessication forced a southerly expansion of the Sahel, sweeping along with it the pastoral and agro-pastoral populations accustomed to living in a semi-arid landscape. In places these groups expanded further south, perhaps only seasonally, coming into contact with indigenous hunter-gatherers and incipient vegeculturalists. The best documented record of such contacts, come from the Savanna of modern Ghana and the sites of Ntereso, Kintampo and Daboya (Davies 1966, 1973, 1984; Stahl 1985; Shinnie and Kense 1989). There, 'Saharan' Kintampo Complex projectile points, stone arm rings, beads, small stone axes and livestock appear in the midst of indigenous Punpun Phase microlithic quartz assemblages around 3500 BP. Subsequently, it would appear that instead of population replacement there was a type of population fusion, as the Kintampo Complex quickly adapted to the subsistence potentials of the savanna-forest ecotone (Stahl 1985).

Elsewhere, there is evidence that West African savanna-forest hunter-gatherers survived as discrete populations until the end of the first millennium AD. Quartz microlithic industries, showing typological continuity from their predecessors, have been dated to surprisingly recent times: Sopie FkBv1, Liberia (2360±125 BP: Gabel 1976), Kamabai Shelter, Sierra Leone (1190±95 BP: Atherton 1972), Nyamanko, Mali (1430±80 BP: Raimbault and Sanogo 1991: 520), Korounkorokalé, Mali (1,020±105 BP: MacDonald 1994) and Kariya Wuro, Nigeria (950±30 BP: Switsur *et al.* 1994). It may be that small, dispersed bands of indigenous West African hunter-gatherers persisted in isolated regions until *c.* 1000 BP, being entirely absorbed into surrounding agricultural populations after that point. Indeed, the oral traditions of modern West African agricultural societies often cite their groups replacement of indigenous hunting peoples upon entering their present lands (e.g. the Soninke myth of Dinga: cf. Dieterlen and Sylla 1992: 65–6). Perhaps the best model for understanding this phenomenon is that presented by Vansina for the acculturation of Central African Pygmy populations within the past 2,000 years by Bantu agriculturalists (Vansina 1990). The more ancient acculturation and replacement of such groups in West Africa has unfortunately left only the merest of traces for us to observe.

POPULATION EVIDENCE FROM PHYSICAL ANTHROPOLOGY AND ROCK ART (12,000–4000 BP)

Both rock art and human remains indicate that several distinct population groups and cultural traditions were active in the Sahara during the Early and Mid-Holocene. French scholarship has placed human remains within three broad classes: 'Mechtoids', Sudanese, and Proto-Mediterraneans (Chamla 1968; Dutour *et al.* 1994). To these we may be able to add a fourth, as yet unassessed,

physical type representing the remains of peoples who lived out in the Ogolian hyper-arid south of the Sahara. To know whether it is necessary to establish such a group we must await the detailed analysis of the human remains from early Holocene contexts at Shum Laka (de Maret pers. comm.), as the early remains from Iwo Eleru are both poorly preserved and of a single individual.

Mechtoids, named after the type population of Mechta-Afalou, represent a grouping of distinctive Terminal Pleistocene populations known from North Africa (Thoma 1978; Dutour 1989). They are distinguished by a suite of characteristics: gabled cranial vault, low rectangular orbits, wide inter-orbital region, massive mandibles with everted gonial angles, and considerable post-cranial robusticity with marked transverse enlargement (Dutour 1989).

Mechtoids are known from the whole expanse of North Africa during the Terminal Pleistocene: from Egypt and the Sudan (Wadi Kubbaniya *c.* 20,000 BP, Jebel Sahaba 14,000–12,000 BP, and Wadi Halfa 11,950–6400 BP) to the Maghreb (Mechta Afalou and Taforalt *c.* 21,000–10,000 BP) (Dutour 1989; Dutour *et al.* 1994). During the Holocene this physical type continued at Columnatan sites in the Maghreb until *c.* 7000 BP (ibid.). Mechtoids have also been identified in the Malian Sahara by Dutour (1989) at Hassi el Abiod and Asselar (both *c.* 7000–6000 BP) and at Cap Juby on the Mauritanian littoral (*c.* 6000 BP). More recent Mechtoid populations are also known from the Recent Holocene at Kobadi (cf. Georgeon *et al.* 1992).

The 'Sudanese' class of physical remains is comprised of skeletons not displaying classic Mechtoid traits and exhibiting the broad characteristics of modern African populations: pronounced alveolar prognathism, flat sagittal contour, and wide nasal aperture (Chamla 1968). 'Sudanese' physical remains are not known from the Sahara or North Africa until the Holocene. Their dated occurrences comprise: Ti-n-Hakatan (post *c.* 9000 BP), Amekni (8700–5500 BP), Meniet (*c.* 5400 BP), Karkarichinkat Sud (4000–3500 BP) and Asaqaru (*c.* 3500 BP) (Chamla 1968; Dutour *et al.* 1994).

The 'Proto-Mediterranean' class of physical remains is comprised of skeletons not displaying classic Mechtoid traits and exhibiting the broad physical characteristics of modern Mediterranean 'Caucasoid' populations: no marked alveolar prognathism, rounded sagittal contour, narrow nasal aperture (Chamla 1968). This physical type is first identified in Africa with the Capsian material culture (*c.* 9000–6000 BP) in the Maghreb. Ferembach (1976) has proposed physical links between Capsian populations and the Natufian populations of the Levant. Although Lubell *et al.* (1984) have challenged this view, a Levantine origin for at least a part of this population remains the Francophone consensus (Camps-Fabrer 1989; Dutour 1989). Verified and dated Proto-Mediterranean remains are scarce in the Sahara: Erg in Sakhane and Kesret el Gani (both *c.* 4500 BP) being the best examples (Dutour *et al.* 1994). Many of the more recent dry-stone burial tumuli of the central Sahara have been implicitly linked with 'Proto-Mediterranean' populations, although we await detailed osteological analyses (e.g. the tumuli of Iwelen, *c.* 5000–2000 BP: Paris 1990).

For our purposes several interesting points are apparent. During the Terminal Pleistocene most if not all of North Africa was inhabited by peoples of Mechtoid physical type. Populations of more gracile 'Sudanese' type become apparent only in the Holocene, although their point of origin is not clear. They could be a development from the Mechtoid type, or the result of intermixing with northwest African or poorly known southerly populations. 'Proto-Mediterraneans' would seem to be absent in the Sahara until the Mid-Holocene. Still, it should be noted that the ultimate discriminatory value of osteometrics and osteomorphology when applied to Holocene Sahara is as yet unclear, since in many cases even these broad types may grade into one another. Indeed, the difficulty of locating Mechtoid traits in modern African populations (cf. Dutour 1989: 276) would argue that the genetic mixing and/or natural drift of these populations over time was substantial.

Rock art provides us with further information on the diverse populations in the Sahara from the Mid-Holocene (Lhote 1970; Dupuy 1988; Muzzolini 1992). Since the 1950s Saharan rock art studies have evolved from the diffusionist ramblings of early explorers to gradually more sober evaluations of the problem. Still, until some form of rock art dating is attempted in the Sahara (e.g. Van der Merwe *et al.* 1987), one must worry that new chronologies and stylistic groupings are mere sophistic triumphs for the field's practitioners.

Lhote (1959: 191–204) put forward four successive periods for the Saharan corpus which have more or less stayed with us until the present day: the Bubaline ('Epi-Palaeolithic' or 'early neolithic' hunters of the extinct giant African buffalo *Bubalus antiquus*), the Bovidien ('neolithic' pastoralists and their cattle), the Caballin ('protohistoric' horses and charioteers), and the Camelin ('protohistoric' camel riders). Relative dating was based upon the association of images with animal depictions, representing beasts with known temporal ranges in the Sahara. But such animal associations are not always present, nor are they without certain circularities of logic (e.g. When did cattle really appear in the Sahara? How long did the giant buffalo or normal African buffalo last in the Sahara?). Muzzolini (1992, 1993a) has challenged this traditional chronology, finding it diffusionistic, stylistically inconsistent and directed by the graphic equivalent of *fossiles directeurs*. In its place he has outlined three distinct regional chronologies for the Atlas, Fezzan and Tassili highland regions, all commencing around 6000 BP, with none including a pre-pastoral phase. Only in the Tassili, with its rich corpus of pictographs, does Muzzolini (1992: 749–52) make use of anthropomorphic depictions for racial correlations: he notes a temporal succession of three pastoral groups in the region, from *le groupe négroide de Sefar-Ozanéaré*, to *le groupe mixte d'Abaniora* (a 'Melano-Europoid' group), to *le groupe europoide d'Iheren-Tahilahi*. All of these groups predate any depictions of horses or camels and thus should have existed before *c.* 3000 BP.

Other writers (Camps 1980; Vernet and Onrubia-Pintado 1994) have utilized rock art depictions from the Atlas and central Sahara, cross-referenced with depictions of plumed 'Libyans' in Egyptian New Kingdom tomb

paintings, to argue for the presence of proto-Berbers in the Saharan highlands by 4000 BP. This is further based upon a correlation between the central Saharan 'Neolithique Recent' material culture (*c.* 4000–3000 BP) and of wide-ranging petroglyphs and pictographs allied in style to what Muzzolini (1992) has termed *le groupe europoide d'Iheren-Tahilahi,* representing plumed warriors with spears (Dupuy 1988). Thus, osteological and graphic evidence may possibly combine to support the entrance of proto-Berbers, presumably of Capsian descent, into the central Sahara at the end of the Middle Holocene (*c.*5000–4000 BP).

Another recurring ethnic link in Saharan rock art literature is that between the Tassili pictograph corpus described by Muzzolini as *le groupe mixte d'Abaniora* and the modern Fulani pastoralists (cf. Hampate Ba and Dieterlen 1966; Lhote 1970; Smith 1993b). Similar hypotheses have more recently been put forward using the petroglyphs of the Adrar des Iforas corpus (Dupuy 1988). Authors have used various attributes of these 'pastoralist' rock art groups to argue this connection: physical similarities between the depictions and the Fulani (Lhote 1970), depictions of rituals still practised in the 1990s by the Fulani (Hampate Ba and Dieterlen 1966; Smith 1993b), the rendering of geometric motifs on the side of cattle in paintings and on pictographs (another modern Fulani practice: Dupuy 1988), and depictions of camp organization as practised by the modern Fulani (Smith 1993b). Although many linguists may find such equations surprising due to the supposed Senegalo-Guinean homeland for Fulfulde (Wilson 1989), there are now genetic reasons to revive an Afroasiatic connection for modern Fulfulde speakers (cf. Excoffier *et al.* 1987 and the next section). One possible model to reconcile these linguistic, genetic and archaeological differences would be to postulate the existence of a coherent pastoral substrate in the Sahel prior to the eastward Fulfulde language expansion of the past 1,000 years. Even the 'core' Fulani could have been a cut-off element of this substrate which had reached the Atlantic coast before the recent Holocene. The nomadic pastoral substrate, which had been depicted in portions of the Saharan rock art corpus, would have been absorbed by this 'Fulani' expansion, retaining portions of its ancient ritual and organizational characteristics.

To the south of the Sahara, in West and Central Africa, anthropomorphic depictions in rock art are uniformly schematic – in essence consisting of 'stick figures' (Gallay 1964; Loumpet-Galitzine 1992; Huysecom 1993). Although the time-depth for this Sub-Saharan corpus has traditionally been thought not to exceed the past 1,000 years, it is interesting to note the broad stylistic and thematic similarity of pictographs and petroglyphs from the Upper Niger to Zaire. Panels over the vast region often share the same numerous components: human stick figures, depictions of lizards (rendered 'from above'), and zoned geometric designs. This could be simple coincidence, but the obvious lack of parallel between this Savanna/Forest tradition and the Saharan corpus would, if they were to share a comparable chronology, argue for two very different spheres of cultural contact.

LANGUAGE PHYLA OF THE SAHARA AND WEST AFRICA (12,000–4000 BP): AN HYPOTHESIS

It is now desirable to advance some putative archaeological correlates for the major language phyla of West Africa. In doing this we must perforce build from present language distributions and genetic data to information supplied by archaeology and physical anthropology. Efforts by geneticists to document the diversity of extant world populations have shown that correlation between genes and language phylum to be good, but hardly absolute (Excoffier *et al.* 1987). We must realize, for example, that many original Niger-Congo speakers may now be speaking Afroasiatic languages, and vice versa. Thus, the cultural and linguistic realities we are attempting to classify have undoubtedly been very fluid over the expanse of time considered.

Archaeology and the lost languages of West Africa

An hypothesis advocating the existence of now disappeared hunting-gathering cultures in West Africa, stemming from coastal and forest Ogolian refuge populations, has already been advanced (see pp. 35–40). In terms of material culture, the LSA (Late Stone Age) traditions of Sub-Saharan West Africa show little evidence of direct cultural contact with the Saharan sphere until the Recent Holocene. Before this time most quartz microlith industries maintain a marked typological consistency, ceramics show little resemblance to Saharan wares, and stone axes are left unpolished or edge ground only (MacDonald 1994; MacDonald and Allsworth-Jones 1994). Likewise, occupations before 4000 BP have been documented only from thinly stratified rockshelters and rare open-air debitage scatters (Stahl 1993). These factors would combine to indicate small, mobile, and isolated bands of hunter-gatherers covering a comparatively immense grassland and forest landscape. What became of this putative people? Why is there not a more tangible trace of them comparable to the pygmies of Central Africa?

The answer to these questions may come from a number of directions. Time for the total assimilation of these populations would be greater in West than in Central Africa, beginning more or less throughout the region by 4000 BP. These populations also would usually have dwelt in more accessible terrain than the Equatorial forest. Thus their 'bolt-holes' would have been fewer. Additionally, one can advance a recently recorded language isolate from north-east Nigeria (Jalaa: cf. Blench, Volume III and 1995a) as more concrete linguistic evidence for a vanished people than has yet come from the Equatorial forest.

Following the model of interactive assimilation advanced by Vansina (1990) for the Central African Pygmies, one may assume a gradual process of absorption for these groups. The most northerly vanished hunter-gatherers, living along the Middle Niger, would have first encountered Saharan immigrants in the Mid-Holocene. They may have learned from them the techniques of ceramic and polished stone manufacture which could have filtered southward

through stimulus diffusion. Others may have acquired livestock. Eventually those groups in the main zone of contact – the modern Sahel – would have become acculturated, and lost their original languages. Meanwhile, more isolated hunter-gatherers or even 'rudimentary' vegeculturalists would have continued in their previous way of life, until successive episodes of aridification drove northern populations into their homelands. Then they, like the Central African Pygmies, would have interacted with incoming groups, acting as guides into this new landscape, until they were economically and numerically overwhelmed. Just how long they continued to survive as a distinctive type of society is unknown, although a date in the late first millennium AD would seem likely. Physically, these groups may have been absorbed evenly into encroaching northern and eastern groups resulting in little genetic trace of their existence remaining. Linguistically, isolates such as Jalaa may represent the sole clue we have to their existence outside of archaeology and 'deep time' oral traditions of vanished indigenous populations (cf. Dieterlein and Sylla 1992)

Archaeology and the Niger-Saharan macro-phylum

When one considers the African populations of the Terminal Pleistocene, the seemingly unified physical type of those dwelling in North African refugia (i.e. Mechtoids) and their eventual cultural assimilation of contemporary populations living in West African coastal refugia, then language homelands for Niger-Congo and Nilo-Saharan between the Maghreb and the Nile Valley are strongly suggested. Further, in light of the Niger-Saharan macro-phylum hypothesis (Blench, Volume III and 1995c), a sort of unity for Niger-Saharan speakers would seem likely during the Late and Terminal Pleistocene (*c.* 20,000–12,000 BP). Such a situation, with a rapid diversification of these languages as populations spread out at the beginning of the first Holocene Wet Phase (*c.* 12,000 BP) would accord with archaeological data much better than, for instance, Ehret's (1993) Nilo-Saharan hypothesis which suggests an early migration from the Nile (the 'Aqualithic' or the early Saharo-Sudanese Neolithic) which is no longer archaeologically supported (see pp. 42–3) and leaves little room for the growth of the now geographically dominant Niger-Congo languages.

In essence a 'Niger-Saharan' model, from an archaeological perspective, would progress as follows. Proto-Niger-Saharan speakers may be represented by the post-Aterian, Mechtoid refuge populations of the North African littoral and highlands (*c.* 20,000 BP). Language diversification could have begun during the Ogolian hyper-arid (20,000–12,000 BP) with its consequent production of population isolates. Undoubtedly language diversification would have accelerated when populations began to expand into the Sahara from North African refugia at the beginning of the Holocene (12,000–10,000 BP). It is proposed that the peopling of the Sahara was thus broad based and did not stem from any particular North African region. The first colonists of the Holocene Sahara would have been arid-adapted hunter-gatherers who would have lived

out the Ogolian on the edge of the vast Terminal Pleistocene Sahara. To the east, similar peoples may have been responsible for the indigenous domestication of African cattle (*c.* 10,000–8000 BP) as an economic adaptation to the climatic uncertainties of a still periodically arid landscape. The economic success of this experiment, and its eventual incorporation of sheep and goats from the Levant, would have allowed the rapid expansion of pastoral populations from 7000 BP. These early pastoral populations may have lost their 'true Mechtoid' physical status by this time, both through dietary change and through inter-breeding with other Early Holocene populations. True Mechtoid populations could have endured as occasional isolates stemming from the original North African immigration.

However, it is premature to attach Proto-Nilo-Saharan or Proto-Niger-Congo labels to any particular vectors within this population expansion. It is apparent from the archaeological record that we are dealing with a great diversity of technological and economic adaptation within these groups. From 7000 BP there are pastoralists, hunter-gatherers and fisherfolk spread throughout the Sahara; some using microliths, some using point-based technologies, almost all using ill-catalogued varieties of ceramics. From 4500 BP groups speaking languages of Niger-Congo and Nilo-Saharan affiliation would have been brought into frequent contact with diverse Afroasiatic immigrants and 'vanished language' speakers to the south as aridity pressed populations into ever greater proximity. The untangling of the Niger-Saharan web, if it is possible, awaits the research of years to come.

Archaeology and the Afroasiatic language phylum

The origin of the Afroasiatic (formerly 'Hamito-Semitic') language phylum has been placed either in the Levant or northeastern Africa (Blench 1993). The ancient Egyptians were Afroasiatic speakers, as are the modern Berber peoples of North Africa, Chadic speakers of the Lake Chad basin, the Omotic speakers of Ethiopia, and the Cushitic speakers of East Africa. Afroasiatic languages appear to have travelled from their 'homeland' up the Nile and into modern Ethiopia and the southern Sudan during the Mid-Holocene (Ehret 1982). It is not the place of this chapter to support any individual hypothesis concerning the specific placement of Proto-Afroasiatic's 'core zone' – for our purposes Levantine or Nile Valley localities suffice just as well.

What does fall within our remit, however, is an explanation of the present distribution of Berber and Chadic languages. Claims for cultural and/or population movements out of northeast Africa have been legion in African archaeology. However, in the preceding sections we have pared the best substantiated cases down to two:

- *c.* 10,000–9000 BP The Capsian of the Maghreb with whom Natufian connections have been postulated (Camps-Fabrer 1989) and which has more definite cultural connections extending at least to the Libyan littoral (McBurney 1967)

- *c*. 5000–4000 BP The '*Leiterband* complex' expansion from the Upper Nile (i.e. Esh Shaheinab) to the Lake Chad basin and further westward (Keding 1993).

The linking of Berber peoples with the Capsian industry (*c*. 9500–6000 BP) and its progeny has been advanced at length elsewhere (Camps 1980; Camps-Fabrer 1989). It remains only to add that evidence from physical anthropology and rock art (see pp. 44–7) has strongly indicated a Proto-Berber presence in the central Sahara from 4000 BP. Archaeologically this would be in line with several 'late neolithic' and 'pre-Islamic' pastoral facies documented by Paris (1984, 1990) in the Sahara of Niger.

It is also tempting to link the expansion of Chadic languages from the Nile Valley with the *c*. 5000–4000 BP dispersal of the Esh Shaheinab '*Leiterband* complex'. Such an expansion would account for the historic presence of Afroasiatic speakers in the Lake Chad basin.[5] Further, there are some other Afroasiatic elements (albeit genetic) which it may be worthwhile to consider in this connection. Two of the most significant exceptions to linguistic genetic correlation found by Excoffier *et al.* (1987: 181–2) are the Fulani (Ful°e) of the Sahel and the Asante and Ewe of the central West African coast, who class genetically with modern Afroasiatic speakers such as the Oromo (Galla). This is a fascinating association, which may provide a clue to many ambiguities in the West African archaeological record, including the *groupe mixte d'Abaniora* rock art corpus and the Kintampo complex of northern Ghana. The former, as indicated above, has been used to argue for the presence of peoples ancestral to the modern Fulani in the Mid-Holocene Sahara. The latter, once dubbed the result of 'Saharan invasion' (Davies 1966) and later considered more the result of bow-wave diffusion (Stahl 1985), may have involved a greater influx of people than has recently been considered. This should perhaps not be surprising in the light of well-documented influxes of Saharan populations into other parts of the West African Savanna from the beginning of the Recent Holocene arid phase (*c*. 4000 BP) (see next section). It must be stressed, however, that it is not proposed that this 'Fulani' or 'Afroasiatic' substrate was composed of a single ethnicity. Despite certain similarities in the correlates or some Recent Holocene material cultures, it is likely that regional adaptations and cross-cultural borrowings were the rule of the day during the emptying of the Sahara. Indeed, if this hypothesis were true, one must accept that this wave of Afroasiatic expansion was quite rapidly acculturated by surrounding groups since no trace of Chadic languages remain in the Volta basin, or from the Middle Niger to the coast.

If we link the Capsian and *Leiterband* 'expansions' to the greater Afroasiatic phylum, then some significant linguistic distance between their populations would be indicated by their apparent lack of archaeological interaction and very different time-depths. Such an hypothesis would give greater support for recent views of the internal structure of the Afroasiatic phylum (Blench, Volume III and 1995b), rather than that proposed by Fleming (1983). Fleming

placed Chadic and Berber in close relation, relatively distant from Cushitic and Omotic, whereas Blench indicates discrete North Afroasiatic (Berber-Egyptian-Semitic) and Cushitic-Chadic alignments.

A RECENT HOLOCENE CASE STUDY: THE INITIAL PEOPLING OF THE MIDDLE NIGER

Due to its rich arable land and pasture, the Middle Niger has served more than any other region in West Africa as a cultural crossroads. It is now home to speakers of Afroasiatic, Niger-Congo and Nilo-Saharan languages. Within the Inland Delta alone, more than ten major ethnic groups are present. However, from the available archaeological data it would appear that the region was only sparsely inhabited before 4500 BP when sites begin to become visible at its margins. Its transformation, both in terms of environment and population, was rapid. By 2000 BP the Inland Delta had contracted significantly, and its initial populations joined or replaced by interlopers from the north and east. Its occupational sequence stands as one of the rare African instances when archaeology, oral traditions, and historical linguistics approach a potential for concordance.

Pre-existing fishing populations

From both the Méma and Gourma regions of the Middle Niger there is good evidence for the presence of specialized fisherfolk between 4000 and 3000 BP (Raimbault and Dutour 1989; MacDonald and Van Neer 1994; MacDonald *et al.* 1994; MacDonald 1996). Their vast shell and bone middens contain the remains of unique globular, sponge tempered pottery decorated primarily with pivoting stylus and dragged or rocker comb motifs, impoverished flake-based lithic industries, polished stone points and bone hooks and harpoons. On the basis of these cultural remains, economic data and the human remains interred in these same middens, a direct connection with the Mechtoid fisher-hunter-gatherer populations of Hassi-el-Abiod has been proposed (Raimbault and Dutour 1989; Georgeon *et al.* 1992).

From as yet unpublished LSA materials collected during surveys in the vicinity of Timbuktu and Gao, it is clear that this cultural complex (the Kobadi tradition) was distributed throughout the length of the Middle Niger by *c.* 4000 BP (Figure 1.2).[6] It has been hypothesized that the relatively recent date of the Middle Niger's initial colonization is due to its perpetually paludial and/or lacustrine geography before this time (McIntosh 1983; McIntosh and McIntosh 1988). If this was indeed the case, then it is logical that the region's first Holocene inhabitants were aquatically adapted populations of the Sahara following the retreating waterways of the Palaeo-Niger. Alternatively, new data from the site of Oundjougou, a palaeolake bed in the Bandiagara region, may suggest a longer term occupation around the southern margin of the Middle Niger by indigenous peoples possessing macrolithic industries, with

a later (*c.* 5000 BP) influx of 'Saharan' populations whose ceramics share some affinities with the more recent Kobadi tradition (e.g. sponge spicule temper, comb and *peigne fileté souple* [cord-wrapped cord] décor; Gallay *et al.* 1995; Huysecom pers. comm.).

Incursions by agro-pastoralists (3500–3000 BP)

In the mid-1990s, data indicated that agro-pastoralists appeared around *c.* 3500 BP on both banks of the Inland Niger Delta (MacDonald and Van Neer 1994; MacDonald *et al.* 1994; MacDonald 1996). In both cases, it would appear that these groups coexisted with resident fisherfolk for some time before the disappearance of the latter's material culture.

In the Méma, the agro-pastoral Ndondi Tossokel facies has been dated to 3084±73 BP at the site of Kolima-Sud (MacDonald and Van Neer 1994). The Ndondi Tossokel facies derives from the Dhar Tichitt tradition, and more precisely from its Chebka and Ariane phases (*c.* 3250–2750 BP) (Munson 1976, 1989; MacDonald 1996). It is characterized by grog or chaff tempered everted rim jars decorated with cord-wrapped cord roulettes and a lithic industry dominated by projectile points. In the Méma, cattle and ovicaprine remains are present in Ndondi Tossokel facies contexts, but the millet agriculture associated with similar ceramics in the Tichitt-Oualata region has not yet been positively identified. Physically, on the basis of a limited corpus of data, the inhabitants of Dhar Tichitt are thought to have been of a *Soudanais* type (Dutour *et al.* 1994: Fig. 2). Ndondi Tossokel skeletal remains have not yet been recovered from the Méma.

In sharp contrast to Kobadi tradition settlements, which were situated within the floodplain beside primary channels or on islets, Ndondi Tossokel facies settlements first appeared on the edge of the Méma's ancient floodplain (MacDonald 1994). On the islet site of Kolima-Sud, however, layers of mixed Ndondi Tossokel/Kobadi occupation overlie 'pure' Kobadi layers potentially indicating a seasonal cohabitation similar to that practised by present-day fishers and pastoralists in the Inland Delta (MacDonald 1996). At some point between *c.* 3000 and 2000 BP both the Kobadi and Ndondi Tossokel material cultures disappear from the region and are replaced by the Faita facies, an entity lacking a lithic component and with ceramic décor affinities to Ndondi Tossokel, which would appear to be associated with the beginnings of iron technology in the region (MacDonald 1994).

A similar scenario has been documented in the southwestern Gourma near Douentza, where another (agro?)-pastoral tradition (Windé Koroji) seems to displace or absorb a pre-existing Kobadi tradition element (MacDonald 1994; MacDonald *et al.* 1994). The Windé Koroji tradition (dated to 3635±90 BP and 3115±195 BP) shares numerous affinities with the slightly earlier Karkarichinkat facies of the Tilemsi Valley (the 'Facies K' of Gaussen and Gaussen 1988): hyper-denticulated projectile points, unusually small polished stone axes, twisted-cord and cord-wrapped stick rouletted pottery, cattle and ovicaprine herding, etc. Such resemblances may indicate that the Windé

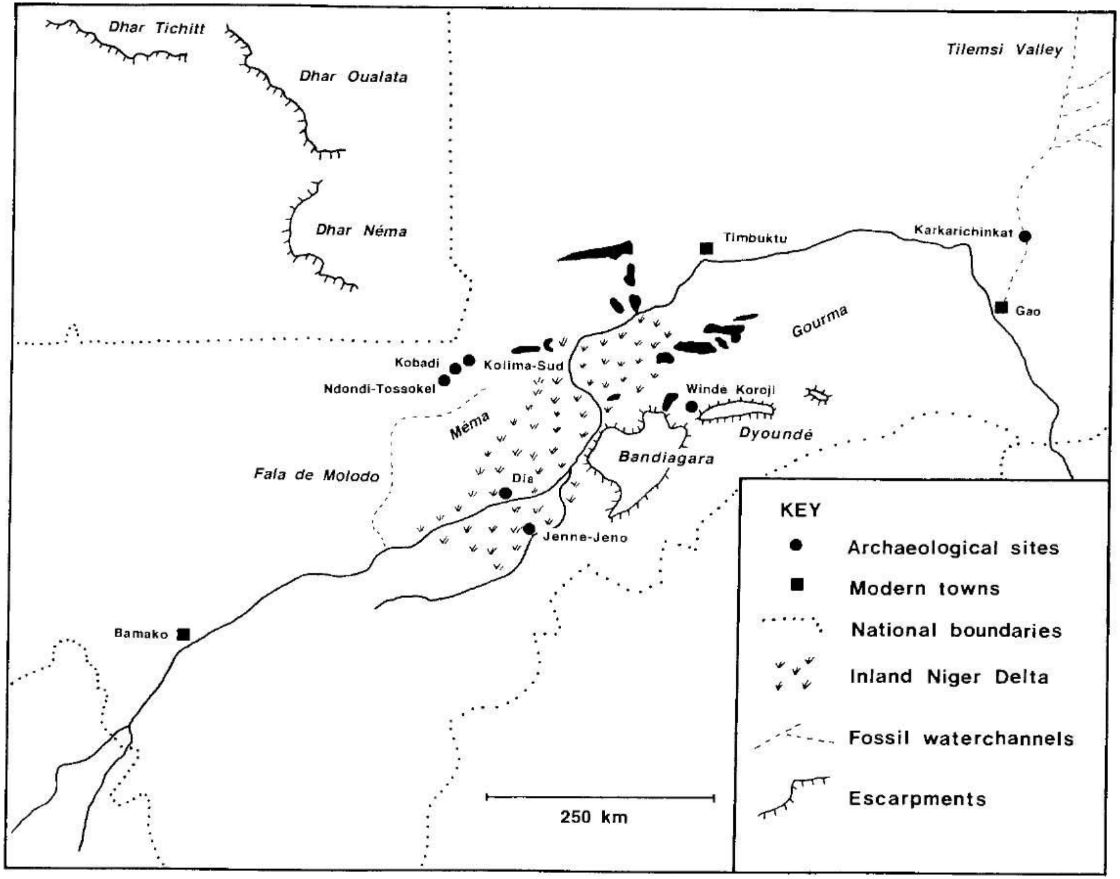

Figure 1.2 Middle Niger and environs (4000–2000 BP)

Koroji tradition was the result of a southwesterly migration by some of the Tilemsi Valley's occupants after that region's virtual abandonment around 3500 BP (Smith 1979). Still, it should be stressed that the Windé Koroji and Karkarichinkat facies are far from being identical. As in the Méma, over time a third facies appears which it is assumed results from a fusion of the previous two (the Zampia tradition, dated on a single determination to 2607±86 BP: MacDonald 1994). Again, there is a much greater stylistic link between the Zampia and Windé Koroji traditions than there is between Zampia and Kobadi.

Oral traditions, linguistics and archaeology (3000–2000 BP)

In the oral traditions of the modern Inland Niger Delta, it is unanimously agreed that the Bozo fisherfolk were the autochthonous inhabitants of the region (Daget *et al.* 1953; Gallais 1967: 78–82). The Bozo's own oral traditions propose numerous and diverse points of origin for themselves, a factor seeming to imply the absorption of immigrant fisher populations with differing genealogies over an unknown length of time (ibid.). At some point before the coming of Islam, however, the arrival of another discrete people from the north is attested by the oral traditions. These people are termed the 'Nono' in the *Tarikh as Sudan* (Es-Sadi, *c.* AD 1650) and in numerous Mande oral traditions. The Nono are identified by Gallais (1967: 79) with the modern Marka rice cultivators of the Inland Delta. The Marka are an amorphous and relatively recent Soninke ethnic creation, whose main criterion for membership is to be Muslim. In the south of the Delta they even share a dialect with a Bozo sub-group (the Sorogo) (ibid.). However, it is interesting to note that Monteil (1932: 30–6) did not assign any modern ethnic equivalence to the Nono, other than vaguely classing them as an ancient part of a broader Soninke lineage, and assigning them a 'Sanhadja' or Maure ancestry.[7] Indeed, Huysecom and Mayor (pers. comm.) in their extensive ethnoarchaeological investigations in the Inland Niger Delta did not meet anyone who would class themselves as a Nono, or who would equate them with any living group. 'Nono', it would seem, no longer exists as an ethnic label.

However we define 'Nono', it remains a term implicitly linked to a pre-Islamic incursion from the Sahel to the Delta. Archaeologically, it is tempting to link this incursion with the Recent Holocene regional sequences discussed in the preceding sections, although additional information would place the Méma, rather than the Gourma, as the heartland of this story. To begin, the Tichitt-Oualata diaspora has long been associated with the Soninke or Proto-Soninke peoples. A case for a dispersion of Proto-Soninke from Dhar Tichitt to the Inland Niger Delta was based by Munson (1980) on various grounds: vestigial Soninke-speaking peoples in the modern Tichitt region (the Aser), architectural similarities between modern Mande and ancient Tichitt compound arrangements, and stylistic similarities between late Tichitt pottery (*c.* 2500 BP) and the wares of modern Diawara (Soninke) potters. Additionally, in both southwestern Mauritania and the Méma, Mande place-names remain

prominent in these regions now occupied by Moor and Fulani pastoralists (e.g. Kolima, one of the Méma's principal archaeological site clusters = the name of the Bozo hero/founder-figure Kolima-Maa). Finally, it is fascinating to note that the earliest inhabitants of the Jenne region were Mechtoids, an apparently viable physical link with the Kobadi tradition (Phase I/II inf. layers of Jenne-Jeno, *c.* 2200 BP: McIntosh 1995: 356).

If we tentatively hypothesize an equation of the Ndondi Tossokel facies and the 'Nono', and the Kobadi tradition with the bulk of the modern Inland Delta fisherfolk, then we are left with a linguistic puzzle. The present-day Bozo, along with the Malinke, Soninke and several other Inland Delta groups, speak Mande languages. The Bozo also share myths and ritual affinities with many of these groups. If we assume that the Ndondi Tossokel facies assimilated the local members of the Kobadi tradition within a Proto-Soninke dialect, then how did the fishing peoples of the upper Inland Delta also come to speak closely related Mande languages? Indeed, the same thought provoked Gallais (1967: 79) to write '*on comprend mal pourquoi [le Nono] aurait abandonné entièrement sa langue d'origine pour prendre celle de peuple autochthone*'.[8] But can we not argue the reverse? Could not an incoming food-producing people, fleeing drought in the north, have had a decisive impact on fishing peoples trying to survive in a similarly declining environment? To paraphrase Gallais's (1967: 78–82) original model for the peopling of the Inland Delta, perhaps the Ndondi Tossokel people (upon encountering stands of wild grains in and around the delta more vast then they had ever encountered in the north) began to practise rice cultivation in the centre of the Delta and continued millet cultivation along its margins (with varying degrees of livestock husbandry persisting). In doing this they interacted closely with the region's indigenes, intermarrying, and absorbing others into their ranks by their economic success. More distant fishing groups would have continued as they were. But soon, they too were pressed by other incoming Saharan populations (perhaps representing other branches of Niger-Congo, particularly Gur). Some were assimilated, but others survived untouched.

Further arguments in favour of Mande linguistic expansion stemming from a recent Saharan immigration, is the wide distribution of Mande speakers in other areas likely to have been affected by the Tichitt diaspora (i.e. in eastern Senegal), and the time-depth of *c.* 4000 BP posited on glottochronological grounds for the Mande diaspora by Dwyer (1989). Additionally, it is interesting to note that Dwyer (1989: 50) places a Soninke/Bozo linguistic divergence at *c.* 2700 BP – a reasonable date if we posit an initial linguistic 'over-printing' at *c.* 3500 BP, with a subsequent divergence of these two groups over time. Still, this is only one hypothesis and there are alternatives. For instance, the Kobadi tradition and the Ndondi Tossokel facies could be posited as both being Proto-Mande. But the radical differences in their ceramic and lithic technology would seem to be indicators of a similarly marked linguistic difference.

If we expand our field of view to include areas outside of the Inland Delta we may see that modern Middle Niger waterfolk are not exclusively Mande

speakers. Along the Niger Bend, there are the Songhai-speaking Sorko fisherfolk and the Gur-speaking Do/Korguey. Interestingly, the Sorko have oral traditions placing Gur-speaking Koromba hunters and Do/Korguey fishers as the indigenes of that region (Rouch 1954; Sundstrom 1972).[9] However, the sharing of pre-Islamic religious traditions between the Sorko and the Do/Korguey may also be used to argue for either a long-term cohabitation of the Niger Bend by these groups, or the existence of a vanished (homogeneous?) Niger fisherfolk substrate of which these groups form a part (Sundstrom 1972: 90). In other words, it is possible that before the 'emptying' of the Sahara (*c*.4000 BP), a single Niger fisherfolk substrate existed (the Kobadi tradition) which was then acculturated along different parts of its length by incoming Niger-Congo speakers.

In searching for a linguistic identity for this substrate one is presented with few clues. A desire to link it with one of the West African 'vanished languages' is hampered by the seemingly Saharan ancestry of the Kobadi tradition's earliest archaeological manifestations (e.g. Hassi el Abiod: Raimbault and Dutour 1989). More likely options would include associations with either the Gur language family or with a (vanished?) Nilo-Saharan language. It is doubtful that this rather complicated puzzle will have an immediate or simple solution. Still, the integration of oral history and linguistic considerations has substantially informed this region's archaeological research.

CONCLUSION

This chapter represents an initial attempt to correlate archaeological findings with biological anthropology and historical linguistics. In making what I believe to be the most parsimonious model, several popular theories have been discarded and some rather daring conclusions drawn. Those two great obscurantists, the 'Aqualithic' and the 'Néolithique de Tradition Saharo-Soudanais,' have been placed once more upon the shelf. I hope these pleadings, as well as those of other authors (Maitre 1972, 1979; McIntosh and McIntosh 1988; McIntosh 1993; Muzzolini 1993b), will put an end to their use. Without them, we shall no doubt come to a more detailed, diversified and accurate notion of the later prehistory of the Sahara and its environs.

Among the new ideas put forth here is the possibility of the linguistic, if not physical, disappearance of indigenous West African hunter-gatherer populations between 4000 and 1500 BP. Allied phenomena are well documented from slightly more recent periods in Central and Eastern Africa (Phillipson 1977b; Vansina 1990). It is hypothesized that the greater time-depth of this disappearance, and the low population densities of these populations, have led to their relative obscurity. Additionally, a new importance has been attached to the Late Pleistocene hunter-gatherers of Sahara's northern rim from modern Chad, through Algeria, to Mauritania. These ephemeral groups along with the broader North African Ogolian refugia populations are thought

to be assignable ultimately to Proto-Niger-Saharan (see Blench, Volume III and 1995c). It is hypothesized that these groups would have comprised both the first hunter-gatherer settlers of the Early Holocene Sahara, and the first wave of indigenous African pastoralists. Regarding North Afroasiatic and the origin of the Berber peoples, this study has inclined to the long-established, but sometimes attacked (cf. Lubell *et al.* 1984), hypotheses of Camps (1980) and Camps-Fabrer (1989) stressing Early Holocene connections with the Levant. A Libyco-Berber penetration of the central Sahara from the Maghreb and Cyrenaica is suggested around 4000 BP. At around the same time, Proto-Chadic speaking peoples are likely to have entered the Lake Chad Basin and the central Sahara from the Nile Valley. Such an expansion is hypothesized to have left a vanished substrate in the Sahel of which the Fulani peoples remain a part – genetically and culturally, if not linguistically.

It should be stressed, with the exception of the specific Recent Holocene study of the Middle Niger, that I have chosen to paint the correlation between linguistics and material culture with a rather broad brush. At the time-scale and geographical breadth we are dealing with, this is as it should be. Indeed, I remain sceptical of both the real possibility and the ultimate value of attempting any correlation between archaeological entities and languages at family level or below until the Recent Holocene. The peopling of West African and the Sahara is a complex issue, whose inherent 'fuzziness' does not deserve to be obscured by the application of overly tidy theories. Language phyla and families, now unknown, undoubtedly disappeared forever in this zone during the Holocene. Groups which originally spoke the language of one living phylum also, most certainly, were sometimes acculturated by those speaking that on a different living phylum. As the work of Excoffier *et al.* (1987) has shown, the correlation of linguistics and genetics is broadly consistent but possessed of many striking anomalies. The archaeological record is sure to be full of many fascinating particularities. Thus, it is essential that we continue to question the durability and equation of genetics, ethnicity, language and material culture.

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NOTES

1 Certain researchers, in their studies, confronted with the immensity of the Sahara, have too rapidly juggled with potsherds and harpoon fragments, tracing great arrows from the Nile to Mauretania and from the Niger to the northern Sahara. Their object was to show us the invasion routes of the people of Esh-Shaheinab, bearers of the harpoon and wavy-line decorated pottery. It goes almost without saying how much this conception of prehistory, dominated by notions of conquest and struggles for influence, owes more to military thinking than to scientific research.

2 The Saharo-Sudanese Neolithic would undoubtedly never have seen the light of day if there had been prior adequate reflection and researchers had first given importance to the study of regional particularities rather than the construction of vast syntheses.

3 All dates given in this chapter are presented as uncalibrated BP. Regrettably, the dates given in the text were run on a variety of materials. Where dates on multiple materials were available from specific archaeological contexts, dates on wood charcoal were given preference.

4 The earliest well-established date for ceramics along the Nile is from Sarourab, *c.* 9400 BP (Ali Hakem and Khabir 1989). However, most dates for the 'Khartoum Mesolithic' and its associated ceramics fall between 9000 and 7000 BP (Mohammed-Ali 1987).

5 The case for this, using specifically linguistic evidence linking Cushitic and Chadic, has been advanced by Blench (1995a).

6 Kobadi tradition materials, as defined by MacDonald (1994, 1996), have been recovered by Timothy Insoll in the vicinity of Gao and by R.J. McIntosh and S.K. McIntosh in the vicinity of Timbuktu. None of these materials, observed in their study collections, has yet been illustrated in publications.

7 Ancestral links posed between the peoples of the western Sahel and Berber or 'Arab' groups, by oral traditions or Islamic documents such as the *Tarikh as Sudan*, must be regarded with scepticism. These are most likely due to a desire of Islamic convert communities to trace a lineage to the prophet Muhammad, and occur very broadly among the Marka, the Peulh, the Hausa and other ethnicities.

8 'It is hard to understand why the Nono would have abandoned their own language to speak that of the indigenous people.'

9 It is on Rouch's (1954) authority that Do/Korguey are assigned to Gur. However, the most recent listing of Gur languages makes no mention of these lects (Naden 1989).

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2 *Neolithic correlates of ancient Tibeto-Burman migrations*

GEORGE VAN DRIEM

THE TIBETO-BURMAN *STAMMBAUM* AND *URHEIMAT*

In terms of number of speakers, the Tibeto-Burman language family is the largest in the world after Indo-European. Yet by comparison little is known of its past. The family tree of Tibeto-Burman has undergone much revision (e.g. Shafer 1955, 1974; Benedict 1972, 1976; Burling 1983; Thurgood 1985; Bradley 1994a, 1994b). Particularly the position of Chinese has been a topic of great uncertainty. Because comparatively little was known of the historical phonology of Chinese, it was assigned to a superordinate node in the family. The language family was originally called 'Sino-Tibetan' and its main branches were Chinese, or Sinitic, and Tibeto-Karen. The Tibeto-Karen branch, in turn, consisted of Karen and the numerous Tibeto-Burman languages. The 'Tibeto-Karen' construct was the result of ascribing too much significance to the syntactic element order of Karen and to other Southeast Asian areal features in which Karen differs superficially from other Tibeto-Burman languages. Similarly, the prominence assigned to Chinese was the result of both a Sino-centric cultural bias and the pioneering state of the art in Old Chinese phonology and Tibeto-Burman historical comparison.

Karen is now generally accepted to be a sub-grouping within Tibeto-Burman. The unusual position of Chinese as one of the two main trunks in a bifurcated family tree also came under scrutiny as more became known about its historical phonology (Bodman 1980). Still little enough was known about Old Chinese and about Tibeto-Burman historical phonology in general that an eminent sinologist could propose the genetic relationship of Chinese to Austronesian rather than, or more closely than, to Tibeto-Burman (Sagart 1990, 1994). Since the late 1980s dramatic advances have been made in the study of Old Chinese historical phonology, most notably Baxter's (1992) methodical Old Chinese reconstruction. What Baxter (1994) modestly describes as 'improvements in Old Chinese reconstruction' also pointed towards a closer relationship between Chinese and Bodic, as suggested by Bodman (1980). Linguistic arguments, including compelling morphological

evidence, have been presented, demonstrating that Chinese and Bodic form a genetic grouping called 'Sino-Bodic', subordinate to Tibeto-Burman (Bodman 1980; van Driem 1995, 1997). In some ways Sino-Tibetan, as a hypothetical phylogenetic node of a family tree, is analogous to Indo-Hittite. A dwindling number of Indo-Europeanist scholars still regard the Anatolian languages as representing one of two main branches of an Indo-Hittite proto-language, with Proto-Anatolian coordinate with Proto-Indo-European. It can be predicted that in a similar fashion, even after a yet greater body of evidence is amassed demonstrating the subordinate status of Sino-Bodic within Tibeto-Burman, 'Sino-Tibetan' will persist for some time to come and, just as what Puhvel (1994: 315) calls 'the "Indo-Hittite" hydra', will continue to sprout new heads even after it has been decapitated and cauterized.

The fundamental regrouping of Sinitic, together with insights afforded by the recent advances in Tibeto-Burman historical comparison, has led to the new Tibeto-Burman family tree shown in Figure 2.1. Lexical isoglosses and comparative work, most recently Sun (1993), lend support to the current hypothesis that the first split in the family is between the Western Tibeto-Burman languages of northeastern India and Eastern Tibeto-Burman, the main trunk of the family.

This *Stammbaum* represents an explicit hypothesis about the chronology and tangled history of ancient Tibeto-Burman population movements. Names of branches of the family refer to the relative geographical position of the groups at the time of their branching and are based on the relative chronology of branching which I have outlined (van Driem 1995, 1997). Other than the well-established and comparatively recent expansion of Lolo-Burmese and Karenic speakers into Southeast Asia, set into motion in the first millennium BC, the pioneering condition of Tibeto-Burman historical phonology and the

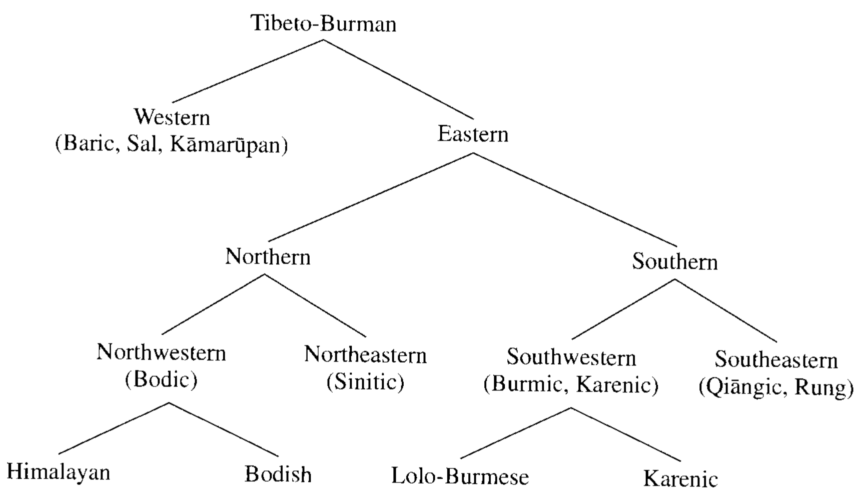

Figure 2.1 The Tibeto-Burman language family

inability to provide absolute dates for the few known sound shifts make it impossible to assign anything but putative dates to the major splits in the Tibeto-Burman family tree. Matisoff (1994: 55), for example, estimates the time-depth of the family as a whole to be six millennia, but this figure is based on no more, or less, than the intuitions of a historical linguist. Similarly, positing the geographical position of the Tibeto-Burman *Urheimat* in the area along the upper courses of the Brahmapūtra, Salween, Mekong and Yangtze is based solely on the geographical centre of gravity argument, a valid but insufficient criterion.

In order to date the population movements which have led to the modern distribution of Tibeto-Burman languages (Figure 2.2), in addition to sound laws, possible correlates in the archaeological record will have to be identified. Here all of the conventional caveats apply. Racial affinities may be an indicator of population movements, but languages can be lost, and a population group can adopt a new language unrelated to its original tongue. The spread of a material culture does not necessarily indicate the spread of populations or of language. Yet race, material culture and language are at least related in a more than just probabilistic way, and, as Mallory (1989: 152) points out, 'the dispersal of a language family is far more reliably measured in terms of languages than in kilometres, since it is the individual languages which form the constituent elements of the family'. Attempts to identify likely archaeological correlates for ancient Tibeto-Burman population movements are by nature speculative, and the following are my speculations on the matter.

WESTERN TIBETO-BURMANS AND THE INDIAN EASTERN NEOLITHIC

Speakers of Tibeto-Burman languages generally happen to be of what used to be unsatisfactorily described as the Mongoloid race in traditional somatology, which were basically crude descriptions of external phenotype. Yet even traditional phenotypic impressionism clearly reveals the greater Himalayan region to be especially complex in terms of prehistoric population movements. Tibeto-Burman speaking populations of predominantly non-Mongoloid racial type can be found in pockets of the western Himalayas, where these groups appear to be the result of a long-term and gradual racial Aryanization of older Tibeto-Burman resident populations. In the Terai belt along the sub-Himalayan foothills, Tibeto-Burman peoples such as the Thāru, Boḍo and Ṭoṭo give the appearance of representing an indigenous South Asian racial type with an admixture of Mongoloid racial stock, the latter presumably reflecting the ancient Tibeto-Burmans who, at any rate, are the linguistic forebears of these peoples.

Assuming that the Tibeto-Burman proto-homeland lay approximately in the language family's present geographical centre of gravity, i.e. in Sìchuān and Yúnnán, the first migration of Tibeto-Burmans out of this area would,

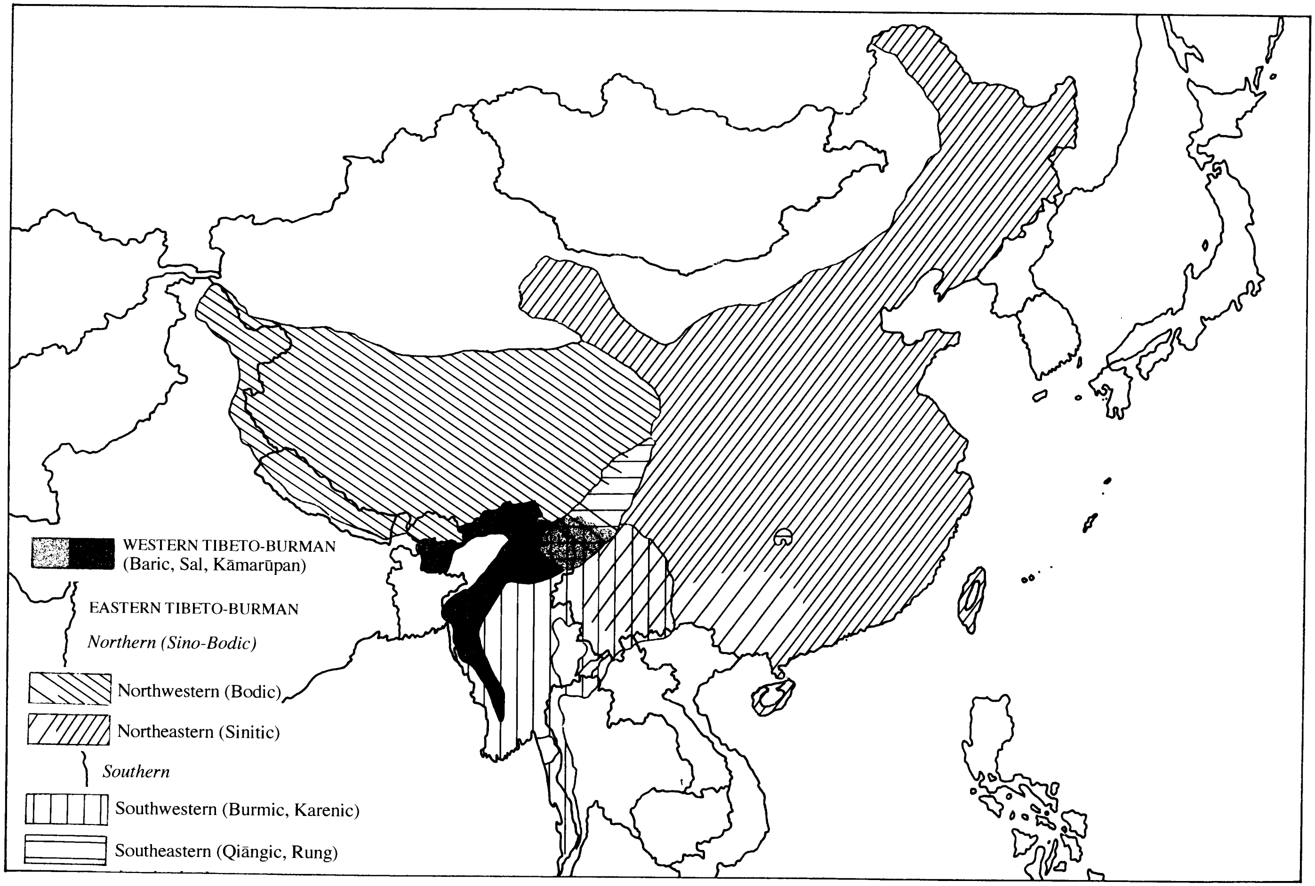

Figure 2.2 Modern distribution of Tibeto-Burman languages. Names of branches of the family refer to the relative geographical position of the groups at the time of their branching and assume the new relative chronology of branching

on historical linguistic grounds, have been the Western Tibeto-Burman migration to the fluvial plains of the lower Brahmapūtra and the surrounding hill tracts. Neolithic implements found in this area represent artefacts of the Indian Eastern Neolithic, for which neither calibrated radiocarbon nor stratigraphic datings exist at present. An early estimate put the Indian Eastern Neolithic at *c.* 2000-1200 BC (Dani 1960) because it was erroneously assumed that metal was required for the manufacture of shouldered axes, or shouldered celts as they are called in the older literature. Although even now 'there is no chronological information' on the Indian Eastern Neolithic (Possehl and Rissman 1992, I: 470), archaeologists judge the Indian Eastern Neolithic to be of great antiquity. Estimates put the earliest phases of this culture somewhere between 5000 and 10,000 BC (Thapar 1985; T.C. Sharma 1989).

Allchin and Allchin (1968: 328) observe that the distribution of Indian Eastern Neolithic tools 'is approximately limited to the areas in which Tibeto-Burman or Muṇḍā languages are spoken'. Robert von Heine-Geldern (1932, 1945: 138) formulated an elaborate migration theory which associated certain styles of stone implements with prehistoric population movements. He identified the Indian Eastern Neolithic and the *Schulterbeilkultur* in particular with the ancient Austroasiatic forebears of the Muṇḍā, whereby he presumed that the Austroasiatics spread into India from Southeast Asia. Although Heine-Geldern's theory is no longer accepted, this particular idea has continued to be influential. However, I believe that there are more plausible reasons to identify the Indian Eastern Neolithic with the spread of ancient Western Tibeto-Burmans. Indeed, the relationship of the neolithic culture of eastern India to those of Southeast Asia is neither as obvious nor as straightforward as it was thought to be when it was first proposed that heterogeneous prehistoric cultures of these regions reflected the distribution and spread of ancient Austroasiatics. Many of the relevant Southeast Asian finds have now become categorized as belonging to the wide-ranging and long-lived mesolithic miscellany collectively known as the 'Hòabìnhian technocomplex' (Matthews 1966; Glover 1973, 1977; Bellwood 1978; Pittioni 1978; Reynolds 1990), after the site at Hòa Bình in Vietnam, the prototype of which was described by Colani (1930). Other early Southeast Asian cultures, such as the Bắc So'n, are likewise not manifestly related in a direct way to the Indian Eastern Neolithic.

The shouldered celt and faceted ground stone axe are the characteristic tools of the Indian Eastern Neolithic, widely distributed in Bihar, Orissa, Assam and Bengal, and the predominant type of pottery is cord-marked grey ware. 'It appears that the shouldered tool type came to Assam through the Cachar Hills Zone from Burma. In the interior it degenerated into the irregular variety as in the Khasi Hills, Brahmapūtra Valley and the Garo Hills Zones', and their irregularity suggests 'that these are rough copies of original specimens' (Dani 1960: 76). This suggests a foreign technology introduced into an area where the indigenous population failed to fully master it. The source of this foreign technology has been identified as Sìchuān. The Indian Eastern Neolithic assemblage is specifically related to the Sìchuān Neolithic

culture, of which it appears to be a local exponent (T.C. Sharma 1967, 1981; Thapar 1985: 44). A cultural complex of great antiquity characterized by shouldered celts and cord-marked grey ware originated in Sìchuān. In fact, the distinctive and fully developed Sìchuān shouldered axe already appears in mesolithic times, and its more primitive mesolithic forerunners are also found in Sìchuān (Chêng 1959: 48). Cord-marked ware is already found in Sìchuān 'in sub-neolithic contexts in association with continuing Mesolithic stone implements' (Chang 1965: 518).

The ground faceted tools of the Indian Eastern Neolithic are unique in South Asia, but such tools are common in East Asia. Regular specimens of ground faceted tools are found predominantly in the eastern part of the Indian Eastern Neolithic, whereas irregular specimens increase in frequency as one approaches the Garo Hills in the west. Other distinctive Indian Eastern Neolithic implements, distinct both in form and in technique of manufacture from their counterparts in the Indian heartland, are the wedges and tanged axes produced by grinding with 'hardly any trace of flaking or battering' (Dani 1960: 76). Of Indian Eastern Neolithic tanged axes and shouldered celts, Wheeler (1959: 89) states that 'the evidence is ample enough to suggest . . . an eastern origin for the Indian series, with a bias in favour of central China'. Surface finds in northeastern India of neolithic axes made of jadeite, a locally unavailable material, evidently represent the importation of implements from neolithic cultures in China. Indian Eastern Neolithic wedges and tanged axes have clear parallels in Upper Burma, Yúnnán and Sìchuān.

These developments allow the hypothesis that manufacturing techniques characteristic of the Indian Eastern Neolithic were introduced into Eastern India by Western Tibeto-Burmans who, at least at the time that they embarked on their migration, were technologically superior to the presumably Austroasiatic populations whom they met up with and with whom they mingled. These neolithic technologies were adopted by the resident Austroasiatics who evidently came to master the techniques, albeit imperfectly. In fact, Austroasiatics would have to be held accountable for the distribution of Indian Eastern Neolithic technologies in regions as far southwest as Orissa, beyond the areas colonized by ancient Western Tibeto-Burmans.

The correctness of the old hypothesis that the ancient Austroasiatics might have expanded into India from Southeast Asia cannot be taken for granted. No decisive arguments have been advanced against the linguistically plausible idea that the Austroasiatic *Urheimat* lay in South Asia itself. The axes with a broad cutting edge found in the Indian Eastern Neolithic could, for example, quite conceivably present technologies which the indigenous Austroasiatic populations already possessed before the advent of the Western Tibeto-Burmans, for, unlike most of the Indian Eastern Neolithic cultural assemblage, these implements have many parallels in other parts of India. The Austroasiatic speaking populations pre-inhabiting this region were the racial stock which the linguistic forebears of the Thāru, Boḍo and Ṭoṭo encountered when they colonized their present habitats. Indeed, the idea that an Austroasiatic substrate

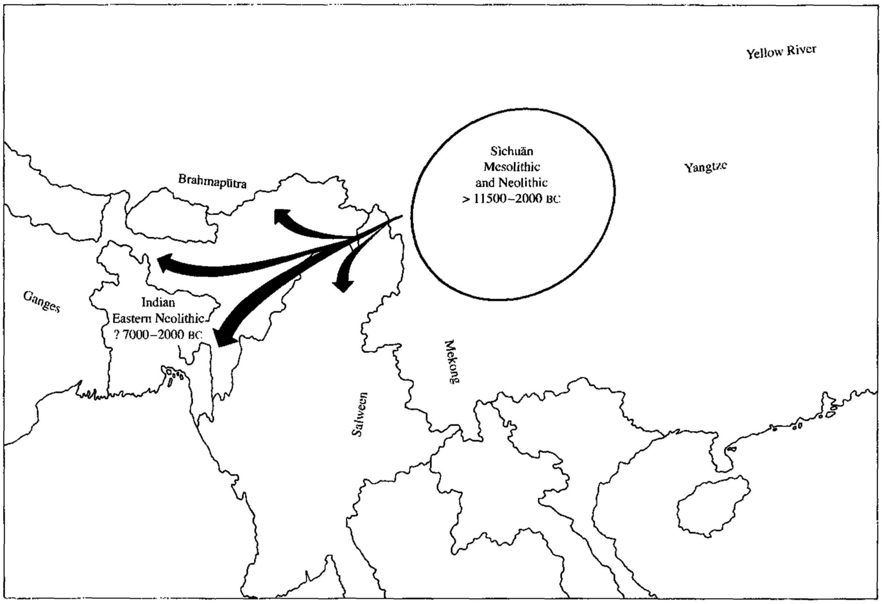

Figure 2.3 Lower Brahmapūtra basin and surrounding hill tracts colonized by Western Tibeto-Burmans bearing the technologies from Sìchuān which were to become known as the Indian Eastern Neolithic, an *Auswanderung* probably set in motion before the seventh millennium BC

may exist in the Tibeto-Burman languages of this region and, for that matter, in Vedic Sanskrit, is not a new one (e.g. Kuiper 1948, 1950, 1954, 1955, 1991). Tibeto-Burman comparative data increasingly appear to support Benedict's view that Western Tibeto-Burman was 'the earliest to split off of common Tibeto-Burman' (Benedict pers. comm., 7 June 1992), and the archaeological correlate for this early split is the Indian Eastern Neolithic (Figure 2.3).

However, neither the Sìchuān nor the Indian Eastern Neolithic have been soundly dated, and alternative hypotheses readily present themselves. If the Indian Eastern Neolithic can be dated to a more recent period, for instance *c.* 3000 BC, which is not all that likely because of its association with cord-marked pottery, then this need not exclude the hypothesis that the Western Tibeto-Burmans were the bearers of Indian Eastern Neolithic technologies into India from Sìchuān. This is because, even if it can be incontrovertibly established that the Western branch was the first to split off from common Tibeto-Burman, the linguistic split could have occurred before the population movement.

EASTERN TIBETO-BURMAN

The remaining main trunk of the family tree, Eastern Tibeto-Burman, split into a Northern and a Southern branch. The most likely archaeological correlate for the Eastern Tibeto-Burman split and the *Auswanderung* of Proto-Northern Tibeto-Burmans would be the abrupt replacement of microlithic technologies of mesolithic communities in the Yellow River basin by the neolithic agricultural Dàdìwān civilization in Gānsù (*c.* 6500–5200 BC) and the contemporaneous, related Péilígǎng and Císhān civilizations on the North China Plain (*c.* 6500–5800 BC and 6000–5600 BC, respectively). The diverse microlithic traditions of mesolithic hunter-gatherer communities in Manchuria, Mongolia, Chinese Turkestan and the North China Plain are unlikely candidates for forerunners to the neolithic agricultural revolution and the sudden flourishing of polished stone technologies and cord-marked pottery which characterize the Dàdìwān and Péilígǎng-Císhān civilizations (Wú 1964; Shào 1984a; Ān 1992).

Neither does southeastern China furnish any likely precursors for these northern civilizations. The Dàpènkēng Neolithic on Formosa (Táiwān) and the Fùguódūn Neolithic of Quemoy (Jīn mén), which Chang (1989: 544) sees as two sub-types of a single neolithic cultural assemblage, and the related neolithic cultures of the Fukian (Fújiàn) coast, such as the Tánshíshān and Xītóu, dated to *c.* 5000 BC (Chang 1989; Lien 1990), represent a cultural tradition quite distinct from the Early Neolithic of northern China (Meacham 1983) and in all likelihood attest to an ancient Austronesian civilization. Dyen (1965: 287) first proposed that the Austronesian homeland lay on Formosa on the basis of the 'high divergence of the Atayalic, Tsouic and East Formosan [i.e. Paiwanic]' branches of the family. Whereas Dyen soon abandoned the Formosan homeland hypothesis, Dahl (1973) recognized the archaic status of the Formosan language groups, and Blust has since become the principal champion of the hypothesis. The comparative linguistic evidence supports an Austronesian homeland on Formosa, which is home to the three main trunks of the Austronesian language family, whereas the fourth branch, traditionally called 'Malayo-Polynesian', comprises all Austronesian languages outside of Formosa, from Southeast Asia to Madagascar, Easter Island and Hawai'i (Blust 1976, 1977). Considerable linguistic and archaeological evidence in support of the Formosan homeland hypothesis has accrued ever since, and Pulleyblank (1983) is among those who interpret the archaeological record of southeastern China in this light. Indeed, the only likely forerunner for the earliest neolithic cultures of northern China lies in Sìchuān.

Southwestern China 'had a long and uninterrupted record of hominid occupation throughout the Pleistocene period, and its postglacial cultural history is above all characterized by the long persistence of native cultural tradition or a complex of native culture traditions that exhibited distinctive features and considerable resistance to rapid and facile assimilation' by the technologically more advanced civilizations which later developed in northern

China (Chang 1965: 517-18). The neolithic technologies of southwestern China represent a gradual, organic continuation of local mesolithic cultures, as in the case of the distinctive Sìchuān shouldered axe and its local mesolithic forerunners as well as the early Sìchuān tradition of polished stone celts (Chêng 1957). The distinctive cord-marked ware which first appears in Sìchuān later appears both in the Indian Eastern Neolithic as well as in the Neolithic Dàdìwān and Péilígǎng-Císhān civilizations of northern China, where it would seem to have been introduced by ancient Tibeto-Burmans. Evidence of early millet cultivation has been found in Sìchuān 'in fully agricultural assemblages' (Chang 1965), and Panicum and Setaria millets were the staple of Early Neolithic agriculture in the Yellow River basin. Unfortunately, Sìchuān, and for that matter southwestern China in general, 'is still at an early stage of archaeological development, but the area's prehistory is clearly of the utmost significance' (Chang 1992, I: 414).

The people who established the Dàdìwān and Péilígǎng-Císhān civilizations preferred settlements on the plains along the river or on high terraces at confluences. No Neolithic complexes older than these two civilizations have been found in northern China, and the Dàdìwān cultural assemblage, first discovered in 1978, is the westernmost Early Neolithic cultural complex to be found in northern China. The Dàdìwān cultural assemblage is represented by sites in Gānsù and Shǎnxī, particularly along the muddy Wèi and lucid Jīng river. The Dàdìwān culture and the contiguous and contemporaneous Péilígǎng-Císhān assemblage along the middle course of the Yellow River share common patterns of habitation and burial and employed common technologies, such as hand-formed tripod pottery with short firing times, highly worked chipped stone tools and non-perforated demi-polished stone axes. The Dàdìwān and Péilígǎng-Císhān assemblages, despite several points of divergence, were closely related cultural complexes.

The stone tool and pottery technologies of Neolithic Sìchuān typify the cultural background of the agricultural pioneers who established the Early Neolithic civilizations which suddenly emerged in the Yellow River basin, 'but there are many characteristics that indicate an early separation from neolithic North China in cultural style' (Chang 1977: 200). Later, Chang (1986: 95) pointed out that, in looking for sources of the Péilígǎng and related neolithic cultures of northern China, the 'crucial area to watch for new finds in contiguous space is the Szechwan hills and the middle Yangtze Valley of Hupei'. Indeed, the Péilígǎng-Císhān and Dàdìwān civilizations had swiftly begun to flourish in the new environment before 6000 BC, whereas the distinctive Sìchuān Neolithic represented the continuation of local mesolithic cultural traditions. The archaeological evidence for linking the Early neolithic of the North China Plain with an emigration of ancient colonists from Sìchuān, although not conclusive, is certainly more than circumstantial. The linguistic arguments provide the most compelling grounds for identifying the first neolithic agriculturalists of the Dàdìwān and Péilígǎng-Císhān cultures with innovators who migrated from Sìchuān to the fertile

loess plains of the Yellow River basin: first, the geographical centre of gravity of the language family and the most probable location for the Tibeto-Burman *Urheimat* based on the modern distribution of Tibeto-Burman languages would appear to lie in Sìchuān. Second, ancient Tibeto-Burmans are the most likely candidates for the people behind both the Sìchuān Neolithic and the cultural complex of the Dàdìwān, Péilígǎng and Císhān civilizations on the North China Plain. Third, the current distribution of Northern Tibeto-Burman languages can be explained by the archaeologically demonstrable dispersal of the Yǎngsháo-Mǎjiāyáo cultural complex, which succeeded the Dàdìwān, Péilígǎng and Císhān cultures (see next section).

Sìchuān is therefore the likely place of origin for the early groups of pioneers who broke away and moved north to settle the fertile fluvial plains of the Yellow River, where they established Early Neolithic agricultural settlements. Not only were these Northern Tibeto-Burman settlers to prove themselves to be technologically innovative in their new habitat, but also they bore with them from Sìchuān to the loess plateau the same technologies, such as polished stone tools and cord-marked pottery, which the Western Tibeto-Burmans had introduced from Sìchuān into northeastern India. After the Eastern split into Northern and Southern Tibeto-Burman, subsequent technological developments were both innovated and introduced comparatively rapidly in the north, whereas relatively egalitarian small-scale agricultural societies are held to have persisted in southwestern China well into the Christian era. This hypothesis places the split between Northern and Southern Tibeto-Burman in the seventh millennium BC, just before the dawn of the Dàdìwān and Péilígǎng-Císhān civilizations (Figure 2.4).

NORTHWESTERN TIBETO-BURMANS AND THE NORTHERN AND SIKKIM NEOLITHIC

The Yǎngsháo Neolithic (5500–2700 BC) succeeded the Péilígǎng-Císhān civilization on the North China Plain, and the Mǎjiāyáo Neolithic (3900–1700 BC) succeeded the Dàdìwān culture in eastern Gānsù and adjacent parts of Qīnghǎi and Níngxià. The period of transition is reflected by the initial stages of the Bànpō and Béishǒulíng Yǎngsháo sub-types, dated around the beginning of the fifth millennium BC (Ān 1979a; Yán 1981; Zhāng and Zhōu 1981; Shào 1984b). The Yǎngsháo and the Mǎjiāyáo cultures represent a distinctly more advanced stage of Neolithic civilization than the relatively smaller sites of the Péilígǎng-Císhān and Dàdìwān, but Chinese archaeological sources point out that a continuity of cultural tradition unites these two stages of development (Xià 1977; Ān 1979a; Zhāng *et al.* 1980).

The development and dispersal of the Yǎngsháo and Mǎjiāyáo Neolithic cultures represent plausible archaeological correlates for the modern distribution of Northern Tibeto-Burman languages, or Sino-Bodic. The Yǎngsháo Neolithic flourished on the fluvial central plains of the Yellow River, but

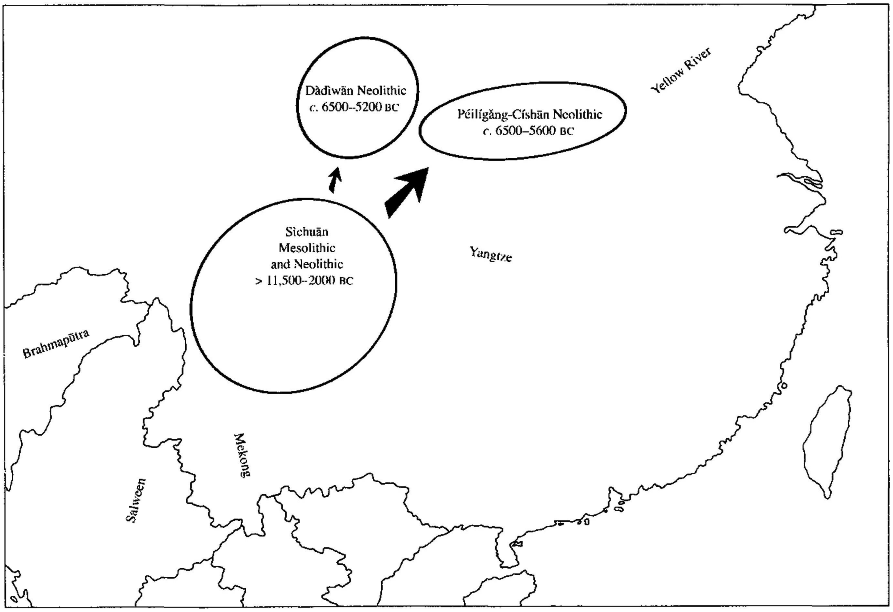

Figure 2.4 The establishment of the Early Neolithic Péilígǎng-Císhān and Dàdìwān civilizations in the Yellow River basin by Northern Tibeto-Burmans before the beginning of the sixth millennium BC

afterwards the centre of gravity of this cultural complex shifted westward to eastern Gānsù, the nuclear area of the more developed Mǎjiāyáo Neolithic, formerly known as the 'Late' or 'Gānsù Yǎngsháo' Neolithic. In addition to this westward shift, Mǎjiāyáo Neolithic culture spread even further westward along the main Inner Asian trade routes across the Himalayas to establish the genetically related Northern Neolithic culture in Kashmir and Swāt (2500–1700 BC), as well as southward through eastern Tibet into southeastern Tibet, Bhutan and Sikkim. Both the far-flung cultural complexes of the Northern Neolithic of Kashmir, or 'Kashmir Neolithic', and the Neolithic cultures of northern Sikkim and of Chab-mdo are colonial exponents of the Mǎjiāyáo Neolithic culture.

The Northern Tibeto-Burman split, i.e. the split of Sino-Bodic into a Sinitic (Northeastern) and a Bodic (Northwestern) branch, could correspond to the differentiation of the probably Proto-Sinitic Yǎngsháo culture on the North China Plain and the probably Proto-Bodic Mǎjiāyáo culture in Gānsù. The spread of Northwestern Tibeto-Burman, i.e. Bodic, languages corresponds to the split-up and dispersal of the Mǎjiāyáo culture outside of its nuclear area in Gānsù. Moreover, the two routes of dissemination of this late neolithic culture would appear to account for the modern distribution of two separate sub-sets of Northwestern Tibeto-Burman language groups, one route being the southward thrust from the nuclear area in Gānsù through northern

Sìchuān into southeastern Tibet, Bhutan and Sikkim, and the other path leading from Gānsù westward through the Karakorum into Kashmir, and subsequently eastward across the southern flank of the Himalayas.

The neolithic culture of Kashmir

> is distinct and stands aloof from that of the rest of India. The dwelling pits, certain tools, particularly the harpoon, the perforated stone harvester and dog burials are alien to Indian Neolithic tradition. The bone tool assemblage including the harpoon has been found at northern Chinese sites. Animal interments, particularly dog burials, recall those of the Ang-Ang-Hse Culture of Manchuria. The burial customs exhibit similarities with the northern and northwestern Chinese and central Asian interments of comparable age. This culture appears to be an isolated development, particularly when we observe that the contemporary well-developed urban Harappa culture in the immediate neighbourhood has had little impact on this culture, although incipient but doubtful infiltration of this urban culture has been observed in the Neolithic ceramics in the form of a couple of pot forms.
>
> (Ramachandran 1989: 52)

Parpola (1994) was the first to propose that the Northern Neolithic could be related to the presence of modern Tibeto-Burman populations in the Himalayas:

> The Northern Neolithic of the third and second millennia BC is considered to be genetically related to the Yang Shao Neolithic cultures of northern China and Mongolia, with which it shares a number of traits. These shared traits include the burial of dogs with their masters, distinctive rectangular stone knives with two holes at one edge, and underground houses which provided shelter against the cold of the winter and the heat of the summer. . . .
>
> The language (assuming there was only one) spoken by the people of the Northern Neolithic may have died out, but not without influencing the later languages of the regions. The many phonological and syntactic peculiarities of the Indo-Aryan Kashmiri, which set it apart from the rest of the Dardic group, point to an extinct substratum language. . . .
>
> One possible candidate for the Northern Neolithic seems worth further consideration, however. The Tibeto-Burman languages belonging to the great Sino-Tibetan language family occupy the mountain ranges bordering the Indian subcontinent on the north and the east. . . . While Tibetan is thought to have come to Tibet from the northeast, the difference between Tibetan and Himalayan languages allows the assumption that the latter may have arrived by a different route, from the northwest, and thus the Northern

> Neolithic may have been Proto-Himalayan-speaking. This is suggested also by the fact that a manuscript relating to the Bon religion of western Tibet has been discovered in Dun Huang, written in the extinct Zhang Zhung language, which appears to have been closely related to Kanauri.[1]
>
> (Parpola 1994: 142)

The apparently archaic nature of Kiranti, East Bodish and other Himalayan languages as well as their demonstrable affinity with Old Chinese are compatible with the suggestion put forth by Parpola. In fact, the cultural unity of the two geographically distant archaeological complexes, the Mǎjiāyáo and Northern Neolithic, provides a precise archaeological correlate to what is suggested by the linguistic data:

> Sino-Bodic appears to be more immediately inspired by common retention than by common innovation, and common retention is only a significant classificatory criterion if there is some other supporting feature, e.g. geographical contiguity. Some major sub-groupings are, in fact, largely based on shared retention and geographical proximity, e.g. Northern and Central Dravidian. On one hand, archaic traits shared between Sinitic and Bodic may just represent a case of Bàrtoli's *norma dell'area meno esposta*, whereby ancestral features are retained in more stable linguistic communities in the periphery without there necessarily being a special phylogenetic link between such peripheral groups, like kentum Indo-European.
>
> On the other hand, the hypothesis posits a Sino-Bodic unity at some point after the break-up of common Tibeto-Burman. Sino-Bodic would have had to have left some traces such as lexical isoglosses, and this is precisely what is suggested by the lexical data presented and by the possible vestiges of a pronominal agreement system in Chinese. In addition to Bodman's impressive list of specific Tibetan-Chinese cognates, more than a score of striking cognate pairs between Kiranti and Old Chinese have been adduced here which suggest that there may indeed exist a significant number of specific Sino-Bodic lexical isoglosses.
>
> (van Driem 1995: 254)

In other words, both the archaeological record and historical linguistic comparison suggest a population movement by bearers of the Mǎjiāyáo culture from Gānsù westward across the Karakorum into Swāt and Kashmir and southward through eastern Tibet into Sikkim and Bhutan. This provides an explanation both for the fact that Tibeto-Burmans inhabit both sides of the Himalayas, the greatest natural land barrier on the face of the earth, and for the close genetic relationship which exists between two geographically distant Tibeto-Burman groups, Sinitic and Bodic (Figure 2.5). It may not be a coincidence that the Mǎjiāyáo and Yǎngsháo Neolithic represent a millet cultivating agricultural

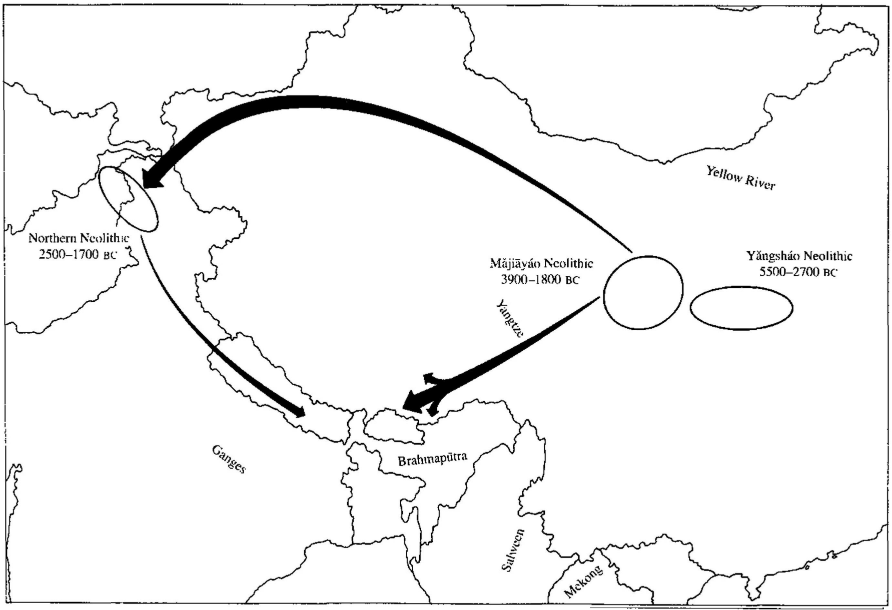

Figure 2.5 The centre of Yǎngsháo civilization moves west to manifest itself as the Late Neolithic Mǎjiāyáo culture. One offshoot of this cultural complex migrates south through northern Sìchuān and eastern Tibet into Bhutan, Sikkim and southeastern Tibet, whereas another offshoot migrates to the southwest across the Himalayas to establish the Northern Neolithic civilization. In this way, Northwestern Tibeto-Burmans people the Himalayas, both from the northeast, colonizing southeastern Tibet and Bhutan (Proto-Bodish, Proto-Gongduk) and establishing the Northern Sikkim Neolithic (Proto-Lepcha), as well as from the west, radiating through the mountains from the Northern Neolithic area eastward at least as far as Limbuvān

civilization and that the modern Tibeto-Burmans of the Himalayas are to this day traditionally millet cultivators as well.

It is quite possible that the factors known to precipitate or facilitate such a movement obtained in the Mǎjiāyáo Neolithic.

> Population movements are determined by three factors. Firstly, there must be a reason to leave one's homeland. . . . periodic exposure to severe stress prompted expansion when the opportunity presented itself. Secondly, there must be a place where life seems to be better in order to make the journey worthwhile. This is the reason to expect migrations toward rather than away from more developed areas such as Assyria in the third and second millennia BC. Thirdly, the cost of the journey must not be prohibitive.
>
> Kortlandt (1990: 134)

Not only in neolithic times has periodic exposure to severe stress been a fact of life in agricultural communities. The occasional occurrence of a severely

poor harvest or ecological calamity would have been highly likely in the area of the Mǎjiāyáo Neolithic, not only because it lies at the chiasma of several highly divergent modern climatic zones, but also because it is known that climatic changes in this region took place in Late Neolithic times. For example, the shores of the Sogō Nūr are strewn with neolithic sites, but its waters are now too saline for human consumption or for use in agriculture. Circumstantial evidence for an exodus from Gānsù is provided by the geographical extent of developmental phases of the Mǎjiāyáo culture. The geographical extent of the Bànshān sub-type (2200–1900 BC) of the Mǎjiāyáo culture is significantly smaller than that of the Mǎjiāyáo sub-type (2700–2300 BC), which preceded it, and represents a contraction of the nuclear area of the culture which roughly coincides with the conveyance of Mǎjiāyáo Neolithic culture to Kashmir.

It is also conceivable that the tidings of prosperity in the west offered prospects of a better life to Mǎjiāyáo Neolithic migrants, and it may be no coincidence that their westward migration coincided with the eastward expansion of Indo-Iranians into inner Asia. A parallel case of convergent population movements is 'the eastward expansion of the "*vorarische oder frühurarische*" Yamnaya culture around 3000 BC and the simultaneous spread of the Finno-Ugric Ural-Kama Neolithic culture to the southwest' (Kortlandt 1989: 79). After the initial westward migration of the Mǎjiāyáo Neolithic culture bearers, their southward thrust into Swāt and Kashmir also makes sense in terms of Kortlandt's second factor, as does the much earlier migration of Northern Tibeto-Burmans from Sìchuān on to the fertile banks of the Yellow River at the beginning of the sixth millennium BC.

Just as the domestication of the horse and the development of the light chariot with spoked wheels reduced the cost of physical mobility to the ancient Indo-Europeans, the cost to Mǎjiāyáo Neolithic migrants of traversing the region which is now Chinese Turkestan was mitigated by the fact that the migration proceeded along an already established ancient trade route. The transmission of neolithic culture across the Himalayas into India from China via the northwestern route not only is an archaeologically attested phenomenon, but also happens to be the most likely route of transmission. When contacts developed between India and China in the first millennium AD, these too passed mainly through Afghanistan and Central Asia.

If the people of the Northern Neolithic were Tibeto-Burmans, as seems likely, then the Himalayas appear to have been colonized by ancient Bodic peoples moving eastward along the alpine tracts from Kashmir as far as eastern Nepal. Epidemiological factors are the probable reason why ancient Bodic groups remained mainly in the hills during their eastward expansion throughout the southern flank of the Himalayas. The malarious jungles on the plains which skirted the Himalayan foothills, rife with human and mammalian parasites and pestilential diseases, were a hostile environment to incursive populations. In fact, epidemiological factors are held to be one of the most significant factors impeding the later Indo-Aryan expansion eastward across the Gangetic Plain (McNeill 1976). There are likewise several epidemiological

reasons for assuming that the Tibeto-Burman *Urheimat* lay at the language family's current centre of gravity (van Driem 1993b: 53–4).

No chronological difficulties are posed by the identification of the Northern Neolithic with the dawn of a Tibeto-Burman colonization of the southern flank of the Himalayas from the west. The Northern Neolithic was contemporaneous with the possibly Dravidian Harappan civilization but remained largely outside of the latter's sphere of influence. The Northern Neolithic was anterior to the several waves of ancient Indo-Iranians migrating south from Central Asia into present-day Iran, Afghanistan, Pakistan and India. The Indo-Iranians are generally held to have been the bearers of the Bronze Age Andronovo culture, which flourished in Central Asia and southern Siberia between the seventeenth and fourteenth centuries BC. In terms of the relative chronology based on the Ghālīgai site in Swāt (Stacul 1969, 1987, 1992a, 1992b; Stacul and Tusa 1977), Stacul (1969: 83) and Parpola (1994: 142, 168) associate the Northern Neolithic with Ghālīgai III (2000–1700 BC), whereas Parpola (1994: 156, 168) relates Ghālīgai IV (*c.* 1700–1400 BC) and Ghālīgai V (1400–800 BC), respectively, to the early Indo-Aryan ('Proto-Dardic = Proto-Ṛgvedic') and late Indo-Aryan ('Late Ṛgvedic' and 'Proto-Nuristani') cultures. Stratigraphically, too, this puts the Northwestern Tibeto-Burman colonization of the southern flank of the Himalayas in advance of the advent of the Indo-Aryans in South Asia.

As for Burushaski, Parpola (1994) remarks:

> Burushaski has strong areal ties with the neighbouring languages Shina and Khowar (belonging to the Dardic group of Indo-Aryan),[2] Wakhi (East Iranian) and Balti (Tibetan). Outside this rather small area its influence has been minimal or relatively recent. For instance in Kashmir there appears to be little trace of Burushaski. It is therefore difficult to link it with the Northern Neolithic found in Kashmir (Burzahom), the Potwar Plateau (Sarai Khola I) and in the valley of Swat (Ghalegay III).[3] Rather, it seems likely that the earliest speakers of Burushaski entered their present homeland from the north after the inception of the Northern Neolithic, and have never gone much further.
>
> (Parpola 1994: 142)

Indeed it appears that the eastward radiation of Northwestern Tibeto-Burman groups from the Northern Neolithic area through the Himalayas must have preceded the advent of the linguistic forebears of Burushaski speakers. It is hoped that comparative studies of the materials already amassed by Lorimer (1935, 1962), Berger (1974), Morin and Tiffou (1989) and Tiffou and Pesot (1989) will shed light on whether there is a genetic relationship between Burushaski and pre-Nostratic languages of Siberia, such as Ket, and to what extent the Burushaski lexicon contains an early Indo-Iranian loan layer.

A separate Tibeto-Burman *Völkerwanderung* by bearers of the Mǎjiāyáo Neolithic culture via the eastern route southward into the eastern Himalayas

has left unmistakable traces in the form of the Bodic population groups of Sikkim, Bhutan and Tibet who are their ethnolinguistic descendants. Bodic, the Northwestern Branch of Tibeto-Burman, is a large and heterogeneous group of languages, and the Bodish sub-group of Bodic (Tibetan, Bumthang, Black Mountain, Dzala, etc.) represents a distinct group whose forebears did not cross the Himalayas in the northwest. Both the archaeological and linguistic data attest to this movement from the Mǎjiāyáo nucleus via an easterly route through eastern Tibet into southeastern Tibet, Bhutan and Sikkim. Archaeological sites which bear testimony of this easterly migration are the findings in northern Sikkim of double-perforated rectangular harvesters and semi-lunar knives and other distinctive artefacts of the Mǎjiāyáo or 'Late Yǎngsháo' Neolithic type, as well as the Late Neolithic sites appertaining to the same cultural assemblage in eastern and southeastern Tibet, particularly around Lha-sa and in Nying-khri and Me-tog counties. Most famous of these is a site discovered in 1977, located some 12 km south of Chab-mdo in Khams in eastern Tibet. Chinese sources (e.g. Xīzàng etc. 1979; Ān 1992) give the name of this site in the sinicized forms 'Kǎruò' and 'Kǎnuò' and report that the toponym denotes 'fort' in Tibetan, possibly mKhar-ru. The site overlooks the rDza Chu, or Mekong, from a high terrace at the latter's confluence with a small lateral tributary, the name of which is also given as 'Kǎruò'.

A.K. Sharma (1981: 83) explicitly excluded the possibility that the Northern Sikkim Neolithic culture was introduced into Sikkim from the north because 'a vast expanse of greatly dessicated lands that separates Tibet from the tool bearing area of northern Sikkim was found totally barren in the Neolithic context'. Yet Sharif and Thapar (1992) do entertain this possibility. In fact sites of Late Neolithic agricultural settlements, notably featuring perforated, polished stone tools, have been found relatively nearby, in the river valleys of southern and eastern Tibet (Wáng 1975; Ān 1992), in areas surrounding the upper course of the Brahmapūtra from the Yar-klungs gTsang-po Valley on eastward to where the river bends to the south.[4] The distinctive Late Neolithic site near Chab-mdo, with its long period of habitation (3300–1800 BC), appertains to the Mǎjiāyáo cultural assemblage (Xīzàng etc. 1979) and lies on the very route of dissemination suggested by the modern distribution of Northwestern Tibeto-Burman language subgroups in the region.

Parpola (1994: 142) observes that the difference between Tibetan and the Himalayan languages is considerable, but some of this distance is an apparent effect of historical changes in Central Bodish. Although Tibetan has a literary tradition of some antiquity, Central Bodish dialects like Tibetan are less conservative than, for example, the East Bodish languages spoken in Bhutan, e.g. Bumthang, Dzala, Black Mountain. East Bodish languages share a number of archaic traits with Kiranti languages of eastern Nepal, spoken by people whose ancestors might have settled the southern flank of the Himalayas from the west. None the less, the comparative linguistic evidence favours the idea that the linguistic forebears of Bodish languages came by a different route. For

example, East Bodish seems to have crossed the Himalayas and entered Bhutan from the north long before the beginning of the Christian era. The presence of Tibetan and other Central Bodish dialects south of the Himalayas is of recent date, for Central Bodish speakers did not begin to cross the passes and descend from the Tibetan Plateau until historical times. The Dzongkha-speaking 'Ngalongpa colonized western Bhutan in the ninth century AD, and the Dränjopa entered Sikkim at about the same time. The Sherpas of Nepal and related groups are even more recent immigrants.

The forebears of the Lepchas are another group which may have crossed the Himalayas from the north and entered Sikkim in neolithic times. This at least is suggested by, first, the discovery of the Northern Sikkim Neolithic cultural assemblage, which is manifestly related to both the Mǎjiāyáo and Northern Neolithic cultures; second, native Lepcha lore which hyperbolically stresses the antiquity of Lepcha habitation in the region; and third, the indeterminate and controversial position of Lepcha within Tibeto-Burman, although Lepcha has evident Bodic affinities, and its special affinities with Old Chinese have also been pointed out (Bodman 1988, 1989).

Xu (1991) makes no mention of the finds in Sikkim but relates the Mǎjiāyáo Neolithic site near Chab-mdo more immediately to the presence of the Northern Neolithic in Kashmir and advances the hypothesis that this cultural complex spread to Kashmir across the Tibetan Plateau. Although this scenario cannot be excluded – certainly the West Bodish groups would appear to have spread westward by way of the Tibetan Plateau, albeit perhaps more recently – the connection of the Mǎjiāyáo Neolithic site near Chab-mdo to the Northern Sikkim Neolithic is a far more obvious relationship geographically.

In short, the linguistic interpretation of the archaeological picture is that the forebears of the Lepcha, of Bodish-speaking peoples and of certain other linguistically divergent Tibeto-Burman groups, like the Gongduk of central Bhutan, settled southeastern Tibet, Bhutan and Sikkim via an easterly route. These Bodic-speaking populations appear to represent a sub-set of Northwestern Tibeto-Burman distinct from the sub-set of Bodic groups whose Neolithic ancestors peopled the southern flank of the Himalayas spreading eastward from Kashmir (Figure 2.5). Future linguistic research will shed more light on the patterns of affinity between Tibeto-Burman groups which make up the complex linguistic patchwork of the Himalayas and thereby enable us to have a more detailed picture of prehistoric population movements which have passed through and around the Himalayas.

SOUTHERN TIBETO-BURMANS, SUMMARY AND CONCLUDING REMARKS

The exodus of Southwestern Tibeto-Burmans into peninsular Southeast Asia must already have begun in the first millennium BC, and the process seems never to have completely come to a halt, as Lolo-Burmese groups have

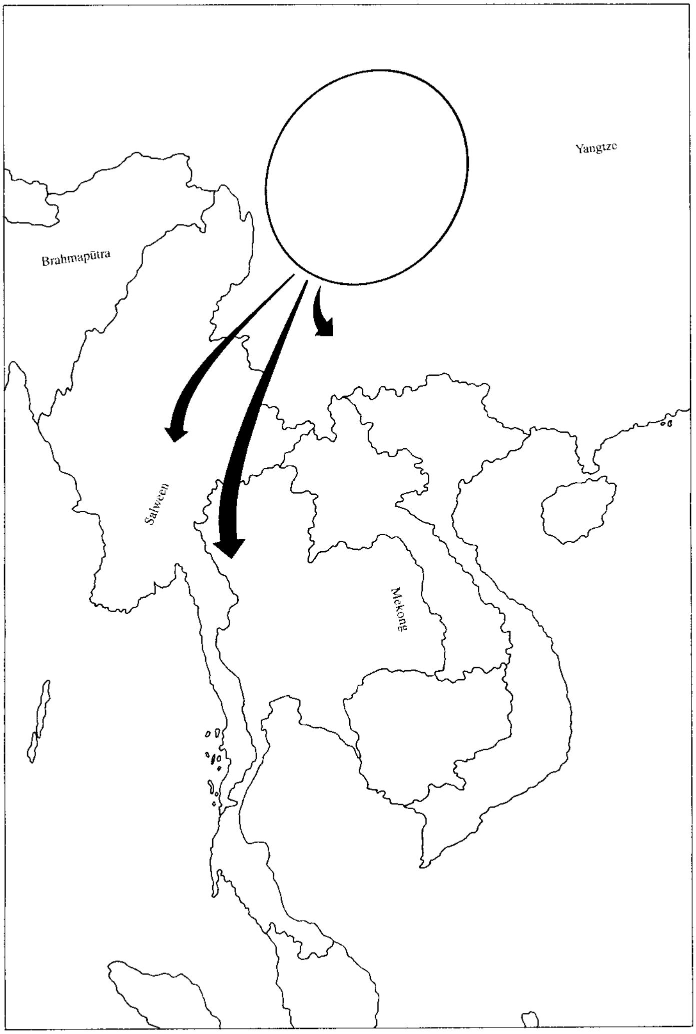

Figure 2.6 The exodus of Southwestern Tibeto-Burmans into peninsular Southeast Asia had begun by the first millennium BC and the process seems never to have been completely brought to a halt, as Lolo-Burmese groups have continued to trickle into Thailand from Yúnnán in recent history

continued to trickle into Thailand from Yúnnán in recent history (Figure 2.6). In the scenario outlined here, the Southeastern Tibeto-Burmans are the group left behind after all other groups have left.

First to emerge from the Tibeto-Burman heartland in Sìchuān were the Western Tibeto-Burman pioneers who introduced the technologies of the Indian Eastern Neolithic and themselves to the Austroasiatic populations of

northeastern India, probably by the seventh millennium BC. Subsequently, the Northern Tibeto-Burmans broke away in the seventh millennium BC and established Early Neolithic agricultural civilizations on the fertile loess plains of the Yellow River, where they introduced the same technologies from Sìchuān which the Western Tibeto-Burmans had previously introduced into northeastern India. The Northern Tibeto-Burman cultures developed into advanced civilizations, and by Late Neolithic times Northern Tibeto-Burman, or Sino-Bodic, had split into a Northeastern (Sinitic) and a Northwestern (Bodic) branch. The Northwestern Tibeto-Burmans spread from Gānsù, bearing the Late Neolithic Mǎjiāyáo culture, via two routes into the Himalayan region. One route took the Bodic bearers of the Mǎjiāyáo culture through northern Sìchuān and eastern Tibet into Bhutan and Sikkim and ultimately across the Tibetan Plateau in the late fourth and early third millennia BC. Another route took these ancient Bodic peoples across the Karakorum, where they established the colonial exponent of Mǎjiāyáo culture known as the Northern Neolithic of Kashmir in the middle of the third millennium BC.

Although Sìchuān yielded the pioneering groups who introduced relatively advanced technologies into northeastern India and established advanced Early Neolithic agricultural communities in the Yellow River basin, there appears not to have been as much innovative technological progress in Sìchuān itself after these emigrations. Instead, neolithic technologies persisted in this region for a long time, and Sìchuān and Yúnnán became culturally peripheral to surrounding areas which had become technologically superior, e.g. the Northern Tibeto-Burman or Sino-Bodic cultures in northern China and the late Neolithic, probably Kadai, cultures of southern China.[5] With the exception of the more recent Southwestern Tibeto-Burman migrations, the geographical dispersal of the main branches of the Tibeto-Burman language family can be identified with neolithic correlates in the archaeological record.

The hypotheses proposed here are a linguist's interpretation of the archaeological record based on the present state of the art in Tibeto-Burman historical linguistics. Future research, both linguistic and archaeological, may necessitate the modification or abandonment of the speculations outlined here. As it stands, the non-random dispersal of unique neolithic cultural assemblages corresponds strikingly to the tortuous branching pattern of Tibeto-Burman phylogeny as suggested by modern comparative linguistic studies. Both the archaeological and linguistic evidence seem to be congruent with one another in suggesting the linguistic intrusions described here. Moreover, our outlook on East Asian archaeology should be devoid of cultural bias. For example, it is both logically true as well as a distortion by simplification to say that the neolithic tumuli of the early Indo-European Kurgan tradition in the Pontic-Caspian steppe are held to represent the linguistic ancestors of the modern Russians and Ukrainians. Similarly, it is probably at once logically true and a distortion of fact to identify the neolithic cultures along the Yellow River with the linguistic ancestors of the modern Chinese.

The Chinese archaeologist Chang (1986) stresses that

> the English word *Chinese* has both a geographical-cultural sense and a linguistic sense. In the latter sense, Chinese means the language spoken by the Han Chinese only. In terms of that interpretation one may question the use of the word to describe the prehistoric interaction sphere, because the Han Chinese language and its speakers were in all likelihood a regional, not a spheric, phenomenon.
>
> (Chang 1986: 242)

Chang proposes to use 'Chinese' exclusively as 'the geographical-cultural label' and 'Hàn Chinese' as 'the linguistic label'. The distinction made by Chang is an essential one, but the labels he proposes can only abet confusion. Just as archaeologists do not refer to the neolithic tumuli in southern Russia as 'Russian', so too should archaeologists avoid the pitfall of referring to neolithic assemblages such as the Dàdìwān, Péilígǎng, Císhān and Yǎngsháo as 'Chinese'. As a linguist, I propose that the term 'Chinese' be used straightforwardly in the meaning which it already has in English, i.e. to denote the Hàn Chinese languages, people and culture. Prehistoric cultural complexes should be referred to by their conventional archaeological designations, after principal sites, e.g. Dàdìwān, Péilígǎng, Císhān and Yǎngsháo. These neolithic cultural complexes in the region now known as China may, in fact, not be Chinese but may variously represent 'Northeastern Tibeto-Burman' (i.e. Sinitic), 'Northwestern Tibeto-Burman' (i.e. Bodic), 'Northern Tibeto-Burman' (i.e. Sino-Bodic), 'Eastern Tibeto-Burman' or just 'Tibeto-Burman' prehistoric civilizations.

THE WIDER ASIAN CONTEXT AROUND TIBETO-BURMAN

The question remains as to by whom shouldered celts and cord-marked grey ware were introduced into southern China and into Southeast Asia, where they occur subsequent to their appearance in Sìchuān, the corded ware horizon is conventionally thought of as passing first through northern Thailand and later to areas further removed from Sìchuān such as Peninsular Malaya and Formosa. In these areas there is no evidence that the spread of these technologies was associated with an early Tibeto-Burman linguistic intrusion or, for that matter, as part of the spread of an identifiable cultural complex. Chang (1986: 242) is loath to give this vast area of disseminating cultural influences a name and so calls it 'Interaction Sphere X'.

The Zèngpíyán and Xiānréndòng cultures of Guǎngxī, 'characterized by coarse cord-marked and incised pottery, associated with mollusk collecting and hunting-fishing' (Chang 1992, I: 413) would now appear to be the oldest representative in southern China of this ancient interaction sphere. The sites in a limestone cave in Guìlín and shell middens in Nánnìng have been dated

to the tenth millennium BC (Běijīng etc. 1982), and Chang (1992, I: 413) identifies the Zèngpíyán and Xiānréndòng Neolithic, 'one of the earliest ceramic cultures in the world', as the predecessor of the Dàpènkēng and the Fùguódūn Neolithic.

Archaeologists and linguists have been highly successful in identifying congruent dispersal patterns documenting the prehistoric dispersion of Austronesian, but it is a far more precarious exercise to attribute prehistoric cultural complexes to the dispersal of language families or genetic constructs with a less well-established status than that of Tibeto-Burman. For example, Austric (i.e. Austroasiatic plus Austronesian), a proto-language conceived by Schmidt (1906), was pronounced 'extinct' by Benedict (1991) but has already been resurrected by Reid (1994), who musters persuasive morphological evidence. The rather limited lexical evidence for Austric is assessed by Diffloth (1994). However, the centre of gravity of Austroasiatic (i.e. Muṇḍā, Nicobarese and Mon-Khmer) lies somewhere around the Bay of Bengal, which happens to be significantly distant from the Austronesian *Urheimat* on Formosa. Yet if historical linguists as meticulous as Reid can conclusively demonstrate that Austroasiatic and Austronesian constitute two branches of a single language family and do not just share etyma exchanged during the early contact within Chang's 'Interaction Sphere X', then this will bear heavily upon the interpretation given to neolithic cultural assemblages and even to the mesolithic 'Hòabìnhian technocomplex' in Southeast Asia.

The centre of gravity of the hypothetical Austric family would lie in Guǎngxī and Tonkin, and the Early Neolithic cultural assemblages as Zèngpíyán and Xiānréndòng readily present themselves as the obvious archaeological correlate of a primordial Austric people, midway between Assam and Formosa. Chang (1992, I: 413) has identified these assemblages in Guǎngxī as the archaeological predecessors of the Dàpènkēng and the Fùguódūn Neolithic, so that the archaeological record would already appear to furnish a ready correlate for a Proto-Austronesian migration from an earlier Austric *Urheimat* centred in Guǎngxī to the Austronesian *Urheimat* on Formosa. The archaeological chronology of this Austric scenario is furthermore compatible with the linguistic chronology for Tibeto-Burman, for it was in earliest neolithic times that the Austrics, to be identified with the neolithic 'Interaction Sphere X' and at least part of the Mesolithic 'Hòabìnhian technocomplex', would have split into an eastbound Proto-Austronesian population and a westbound Proto-Austroasiatic population. This allows the Proto-Austroasiatics ample time to reach Assam before the Western Tibeto-Burman incursion and the spread of the Indian Eastern Neolithic. If the homeland of Austroasiatic were established in Assam at this time, then the ensuing Western Tibeto-Burman *Einwanderung* into the lower Brahmapūtra river basin could have been the impulse which led to the split-up of Austroasiatic, pushing the Proto-Muṇḍā into the South Asian heartland and driving the Proto-Mon-Khmer and the Proto-Nicobarese into the hill tracts, whence they ultimately spread into Southeast Asia and across the Andaman Sea.

Geographically closer to Formosa, however, lies the *Urheimat* of the Kadai language family in southern China, and Benedict (1975) has posited a phylogenetic super-family called Austro-Tai, consisting of Kadai, Austronesian and Hmong-Mien (Miáo-Yáo). Benedict's linguistic archaeological research suggests that the Proto-Austro-Tai lexicon was rich in terms associated with rice agriculture and that some Chinese terms for rice agriculture are loans from Austro-Tai. Evidence of early rice cultivation has been found in Sìchuān (Chang 1965: 518), but the earliest datable evidence of rice agriculture in China dates from the sixth and fifth millennia BC and is associated with the Péngtóushān and Hémǔdù cultures, which could indicate that this staple was first domesticated in Húnán along the middle Yangtze (Yán 1989, 1990) and around the Yangtze delta (Ān 1979b). These locations fit in with the theory that the initial propagators, if not the first domesticators, of rice in East Asia were ancient Kadai, Austronesians or even Austro-Tai.

Yet Benedict's Austro-Tai and Schmidt's and Reid's Austric are not necessarily mutually exclusive hypotheses, for both might embody part of the truth. What I propose to call the 'Greater Austric' family, comprising Austroasiatic, Austronesian, Kadai and Hmong-Mien (Miáo-Yáo), would be one way of compatibly uniting both theories, but at the present comparative state of the art, this Greater Austric hypothesis lies wholly within the realm of conjecture. More compatible with the conventional Austric theory is Thurgood's (1994: 345) argument that the Austronesian etyma found in Kadai 'are neither inherited on the one hand nor mere look-alikes on the other', but result from ancient Kadai 'borrowing from an early (pre-)Austronesian source and that the contact occurred in southwestern China and predated the Austronesian movement out onto the islands'. In view of the lexical borrowing which appears to have taken place between these two ancient linguistic stocks in the neolithic interaction sphere of southwestern China, it is probable that crops and agricultural techniques were likewise exchanged across boundaries of linguistic affinity.

Yet conventional wisdom on rice is perhaps represented by the view that rice agriculture is of South Asian origin, with the oldest evidence of domesticated rice dating from the sixth millennium BC, and perhaps earlier, from the Middle Ganges basin (Glover 1985; G.R. Sharma 1985; Haudricourt and Hédin 1987: 159–61, 176), where one still finds the greatest natural diversity of *Oryza sativa* (Chang Te-tzu 1983). From the Ganges, the irrigation agriculture of rice spread northward to the Northern Neolithic cultures by the beginning of the second millennium BC and in the fifth to third centuries BC was carried by sea from Gujarat, where rice was already cultivated in Harappan times, to Ceylon and Tamil Nadu (Parpola 1994: 9, 172). However, if rice agriculture was known in northern India from such an early date and was introduced to the Dravidian south only in the Christian era, the possible significance of these circumstances must be carefully weighed by those who propose that the Harappan civilization was Dravidian and by proponents of the Elamo-Dravidian theory, which holds that Elamite and Dravidian are related.

Caldwell (1856) was the first to propose a relationship between the Dravidian languages and the language of the freshly deciphered Elamite inscription at Behistun, although this tongue was known to Caldwell as 'the language of the Scythians of the Medo-Persian empire'. The chief proponent of the Elamo-Dravidian theory is McAlpin (1974, 1975, 1981), who renamed the hypothetical language family Zagrosian after the Zagros mountains of southern Iran. Hunter (1934) already pointed out that the Indus script of the Harappan civilization 'bears a close resemblance' to the older Proto-Elamite pictographic script. Fairservis (1992) believes that the Harappan civilization was Dravidian and derives the Indus script from the Proto-Elamite pictographs (Fairservis 1976). Yet Proto-Elamite script fell into disuse after Gutaean barbarian highlanders from beyond the Zagros overran Elam and conquered Sumer and Akkad in the twenty-third century BC, and this leaves us with a gap of centuries which Parpola (1994: 53) attempts to bridge by suggesting that 'the source of inspiration' for the Indus script could have been 'an as yet unknown variety of the Proto-Elamite script that may have been used somewhere within the Elamite realm'. Texts in Proto-Elamite script have been found as far east as Sīstan near the Afghan border (Lamberg-Karlovsky 1972a, 1972b, 1978).

McAlpin (1981) believes that Zagrosian broke up into Proto-Elamite, Proto-Brahui and Proto-Dravidian in the fifth millennium BC, and the evidence which he adduces includes lexical correspondences, shared case endings in nouns, derivational suffixes and other reconstructible common morphological features. The pronominal systems appear related, although Dravidian pronouns would be partially innovative. More cogent evidence is the shared unique 'appellative' system, whereby personal endings are attached to parts of speech in the syntagma other than finite verbs. As one would expect, this agreement system turns out to be more conservative than the system of free pronouns. Although the Elamite and Dravidian verbal systems share morphological traits, the hypothetical Zagrosian conjugation has left only traces in the Dravidian verb. No consensus has been reached, but the exhaustive Elamite dictionary (Hinz and Koch 1987) provides a richer source for future lexical comparison than any which McAlpin had at his disposal.

An idiosyncratic version of the South Asian origins theory of rice is that rice agriculture began in the Northern Neolithic. This view is held by Nakamura (1993: 53), who contends that the 'remains of rice and . . . grain impressions and silicized remains of rice on potsherds' found by Stacul in Swāt periods II and III (2180–1950 BC) are the oldest incontrovertible evidence of rice cultivation, whereby Nakamura categorically dismisses earlier radiocarbon datings as unreliable and criticizes Chinese radiocarbon datings in particular. On the basis of this evidence and his interpretation of other facts, Nakamura contends that 'rice cultivation started from the northern subcontinent, combining the agricultural techniques of the ancient Middle East and the rice plants of the Sino-Japanese floral region' (1993: 53).

A fashionable school of archaeological thought both underrates linguistic palaeontology and somewhat naively interprets the dissemination of language

families across vast expanses as connected with the incremental wave-like advance of neolithic farming. However, languages are not typically spread by sedentary agriculturalist populations but by mobile groups, whether the immigrants form a dominant elite or a numerically strong underclass. Certainly, rice agriculture, whichever group innovated the practice, must have been adopted by different groups of unrelated ethnolinguistic affiliation in order to account for its spread throughout South and Southeast Asia and across southern China. Attempts to relate the spread of rice agriculture to the spread of an entire language family like Sino-Tibetan, as Pejros and Shnirelman have done (Ch. 16 in this volume), are unfruitful for this reason. It is an altogether different matter when the introduction of a specific agricultural technique or crop acts as a tracer for an incursive population group and can be shown to coincide with a prehistoric or recorded linguistic intrusion.

Rice agriculture is held to have been introduced into Japan by the bearers of the intrusive Yayoi culture before the first millennium BC, and possible evidence of rice agriculture is found dating from the second millennium BC during the final Jōmon period (Hudson 1990). The physical anthropological evidence is that a gracile immigrant population soon outnumbered the more robust and less populous indigenous early potters of the Jōmon culture. Benedict (1990) is a proponent of the idea that the advent of the earliest Yayoi culture bearers is to be identified with the influx of an ancient Austronesian population; this idea is one of the ingredients of his Austro-Japanese hypothesis, whereby Japanese and Austronesian form a genetic grouping known as Austro-Japanese, which in turn forms a genetic grouping with Kadai known as Austro-Kadai. Austro-Kadai and Hmong-Mien (Miáo-Yáo) together form Benedict's newly redesigned Austro-Tai superfamily. According to Benedict's hypothesis therefore, Proto-Japanese was first brought to Japan by the bearers of the Yayoi culture, who introduced not only rice agriculture but also themselves and their language, first in Kyūshū and gradually further northward.[6]

More linguistically defensible than the Austro-Japanese theory is the conventional idea that Japanese is genetically Altaic, a theory first defended on linguistic grounds by Philipp von Siebold, who wrote a Japanese grammar, compiled a bibliography of scholarship to date on the Japanese language (1826), and on the basis of comparative studies concluded that the Japanese linguistically derive from a 'zoogenaamden Tartaarschen volksstam', exhibiting the closest linguistic affinity with the 'Mantschoe-Tartaren' (von Siebold 1832). Boller was the first influential proponent of von Siebold's theory, and since Boller's (1857) *Nachweis* a considerable body of analysed evidence has been amassed in support of the Altaic affiliation of Japanese.

The Altaic population which brought the Proto-Japanese language to Japan arrived long after the ancient Austronesians of the early Yayoi intrusion. Miller (1980) associated the propagators of a characteristic type of Middle Yayoi comb-pattern pottery, which has earlier analogues on the Korean peninsula, with the arrival of the earliest speakers of Proto-Japanese in Japan just before

the beginning of the Christian era. By itself Middle Yayoi comb-pattern pottery might be as insufficient an indicator of prehistoric immigration as Heine-Geldern's *Schulterbeilkultur* was, but archaeological advances suggest that the comb-pattern pottery could be but one element of an intrusive cultural complex and have rendered the Middle Yayoi an attractive correlate for an Altaic linguistic intrusion. Sites increase dramatically both in number and in distribution in the Middle Yayoi period, and Middle Yayoi culture expanded rapidly and spread into eastern Honshū. An early Altaic population with Manchu-Tungusic affiliation may plausibly be identified with the people who made their appearance in Japan as the bronze and iron weaponsmiths and tumuli builders of the Middle Yayoi. The Proto-Japanese speakers of the Middle Yayoi interred their dead at mounded burial sites or *funkyūbo*, which were the predecessors of the mounded tombs of the Kofun period. To say that Japanese is what became of 'an Altaic language in the mouths of ancient Austronesians', as I have suggested (van Driem 1993a; 331), is to rephrase the old idea that the Austronesian lexical component in Japanese is a substrate influence dating from the time that the Altaic bearers of the Proto-Japanese language first settled in Japan.

There has been much speculation about the hunter-gatherer culture of the Jōmon period and the Ainu of northern Japan, and some associate the difference between the robust Jōmon and gracile Yayoi racial types described by physical anthropologists with the physical differences between the Ainu and the Japanese which have been abundantly reported both in scholarly and popular sources ever since Brouwer (1646: 98–99). Early toponymical studies by Chamberlain (1887a) and Batchelor (1925) established that the vast majority of place names on Hokkaidō and a large number of place names in northern Honshū are originally Ainu toponyms, of which those ending in *-betsu* or *-be* (< Ainu *pet* 'river') and in *-nai* (< Ainu *nai* 'stream') are but the more conspicuous. Yet it may be that the origins of the Ainu must be sought in the north, as first argued by von Siebold (1858).[7] In fact, it is in the north that the possible linguistic relatives of Ainu are usually sought, for the most likely candidates are a problematic group of languages known collectively as Palaeosiberian or Palaeoasiatic, i.e. Gilyak (Nivkh) and the Luoravetlan languages Kamchadal, Koryak and Chukchee. Attempts to tie Ainu to Altaic (e.g. Patrie 1982) are not convincing, and one of the most viable hypotheses to date remains von Siebold's (1858) idea that Ainu is a language isolate, for which he advanced the argument, still valid today, that Ainu lexical roots appear to exhibit no demonstrable affinity with those of other languages.[8]

The prominence of the bear cult in Ainu culture marks its hyperborean propinquity. Vivid first-hand accounts of Ainu bear festivals provide details of the ritual culminating in the slaughter and consumption of the bear's fresh blood, liver, brains and eyeballs (Scheube 1880; Pilsudski 1909, 1914; Maraini 1991), and popular and second-hand accounts abound (e.g. Verneau 1894; Acherma 1906; Oka 1930). Ainu bear ritualism has been described in detail by early researchers (e.g. Chamberlain 1887b; Batchelor 1892, 1901, 1927;

Starr 1904), and the Ainu bear cult has been compared with the bear ritualism of Uralic, Altaic, Palaeosiberian and Amerindian peoples (e.g. Hallowell 1926; Paulson 1965). Yet it can be no coincidence that bear ceremonialism occurs in the northern parts of Eurasia and North America where bears happen to be most prevalent, nor is it surprising that the slaying of a large-bodied predator as awesome as a bear and yielding as copious a feast of meat as a bear would become ritualized in many arctic and subarctic hunter-gatherer societies. None the less, not only does the Ainu bear cult point to a northern provenance, but also the specific similarities between the elaborate Ainu and Gilyak bear cults are highly suggestive in light of the linguistic hypothesis that Ainu is a Palaeosiberian language. Whether northern origins, primordial residency on Hokkaidō and Sakhalin or even widespread distribution of prehistoric Ainu throughout Japan can ever be established, it might not ever be possible to ascertain the linguistic affinities of the hunter-gatherer peoples who bore the successive phases of Jōmon culture for over ten millennia.

Just as inconclusive as the attempts to classify Ainu are current linguistic attempts to classify certain language groups in East and Southeast Asia, such as Hmong-Mien (Miáo-Yáo) and even Austronesian. Most notably, Sagart champions the Sino-Austronesian hypothesis ('Chinese plus Austronesian', or 'Chinese plus Tibeto-Burman plus Austronesian'). The phylogenetic constructs Austric, Austro-Tai, Greater Austric and Sino-Austronesian are outdone in terms of intrepid speculation by Sagart's (1994: 303) 'expanded Austric', which consists of 'Sino-Austronesian' and Austroasiatic and also possibly includes Miáo-Yáo (Hmong-Mien) and Kadai. A number of 'direct Proto-Austronesian-Proto-Tibeto-Burman comparisons not involving Old Chinese, or with better semantic agreement between Proto-Austronesian and Proto-Tibeto-Burman' have lead Sagart (1994: 303) to concede that certain facts now 'render less likely the possibility that the material shared by Old Chinese and Tibeto-Burman reflects a contact situation. They suggest that Tibeto-Burman languages may stand closer to Chinese (and to Proto-Austronesian) than I had originally assessed (Sagart 1990).'

From the vantage point of Tibeto-Burman scholars, of course, any genetic relationship between Austronesian and Chinese would have to be at the level of the two proto-languages, i.e. between Proto-Tibeto-Burman, as it has now been redefined, and Proto-Austronesian. On the other hand, if the lexical correspondences between Proto-Austronesian and Tibeto-Burman which Sagart (1994) adduces do indeed reflect cognate etyma, they might represent early loans and would constitute linguistic evidence of early contact between ancient Sino-Bodic and ancient Austronesian peoples. The obvious archaeological correlate for such prehistoric exchange is the Lóngshān cultural horizon, which Chang (1986) calls an 'interaction sphere', emerging in the fourth and third millennia BC and connecting coastal cultures from north to south, viz. the Dàwènkǒu cultural assemblage in Shāndōng, the Qīngliángǎng of northern Jiāngsū, and the Mǎjiābāng of the Yangtze delta.

Quite logically, Sagart looks to this Lóngshān interaction sphere for an archaeological correlate of his early 'Sino-Austronesian' people. However, the chronological predecessors of this elongated cultural interaction sphere along the Chinese coastline lay south of the Yangzi delta, i.e. the earlier Hémǔdù culture on the Hángzhōu Bay in Zhèjiāng, the Dàpènkēng Neolithic of Formosa, the Fùguódūn Neolithic of Quemoy, and related neolithic cultures of Fukien of the fifth and early fourth millennia BC. The Lóngshān coastal interaction would therefore appear to have *ensued* upon a northward expansion of Proto-Austronesian culture from its ancient homeland in southeastern China, and this northward expansion of early Austronesians would have brought them into contact with early Northern Tibeto-Burmans. Linguistically too, early contact influence between the two language families, Austronesian and the redefined Tibeto-Burman, remains a more plausible idea than genetic unity, and the Lóngshān interaction sphere is the obvious candidate in terms of time and place for early contacts between ancient Austronesians and ancient Tibeto-Burmans, particularly the Dàwènkǒu Neolithic of Shāndōng with its well established ties both with the other coastal cultures of the Lóngshān interaction sphere as well as with the ancient Northern Tibeto-Burman Yǎngsháo Neolithic civilization.

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NOTES

1 Kinnaurī is a Tibeto-Burman group of languages of the Bodic or Northwestern branch, spoken in Kinnaur district of Himācal Pradeś in the Indian western Himalayas.

2 The term 'Dardic' is an old-fashioned term. The sub-grouping of Indo-Iranian languages in the northwest of the subcontinent is still poorly understood, and few modern linguists have undertaken to perpetuate the tradition of Morgenstierne (1973). Ethnological data and linguistic maps of the area are provided by Fussman (1972), Jettmar *et al.* (1975), Edelberg and Jones (1979) and Masica (1991); cf. also Allchin and Allchin (1982), Allchin and Hammond (1978).

3 'Ghalegay I' in Parpola (1994: 142) should read 'Ghalegay III' (Parpola pers. comm. 20 February 1995; cf. Parpola 1994: 168). I have corrected this misprint in the quoted passage.

4 Undated neolithic implements have been found in southern Tibet as far west as gNya'-lam, which lies approximately 100 km northeast of Kathmandu (Dài 1972).

5 The term 'Tai-Kadai' is used by some, although Benedict, who coined the term, tells me that he prefers the shorter 'Kadai'.

6 In Chapter 15 in this volume, Vovin, who has argued against Benedict's Austro-Japanese hypothesis (Vovin 1994), doubts the existence of any Austronesian substrate influence on the Japanese lexicon and instead advances the improbable hypothesis that the Yayoi civilization was Austroasiatic, an idea made even less plausible by Vovin's acceptance of a Southeast Chinese coastal origin for the Yayoi culture. The linguistic evidence for this hypothesis is three Japanese rice-related terms to which Vovin assigns unconvincing Austroasiatic etymologies.

7 Von Siebold (1858) concludes his deliberations on the provenance of the Ainu with the words: 'De slotsom van deze onze gissingen komt hierop neder: op gelijke wijze als in voorgeschiedkundigen tijd de *Itülmen*, de oudste bevolking van Kamtschatka, naar dit schiereiland gekomen zijn, en later door eenen anderen volksstam opgevolgd en tot aan het zuideinde voortgedreven is geworden, is het ook waarschijnlijk, dat in nog veel vroegeren tijd ook langs de *Amur*, de *Aino*-stam zich allengs over de zoo digt bij het vaste land gelegen eilanden *(Jezo,* de *Kurilen* en *Krafto)* uitgebreid heeft' (1858: 380). ('The conclusion of our conjectures can be summed up as follows: just as the earliest population group of Kamchatka, the Itelmen, arrived on this peninsula in prehistoric times and were subsequently followed up by another population group and driven to the southern tip, it is probable that the Ainu so too may have, in an even earlier period, gradually spread along the Amur to the islands which lay so near the mainland, Hokkaidō, the Kuril Islands and Sakhalin.')

8 Von Siebold's (1858) statement on the genetic position of Ainu as a language isolate reads: 'Ofschoon zich de *Aino*-taal door den gemeenzamen omgang met een beschaafd volk veredeld heeft, zoo bleef dezelve echter haar oorspronkelijk karakter behouden en kenmerkt zich als eene eigenaardige en zelfstandige taal, die met geene van de naburige landen eenige overeenkomst heeft voor zoo verre de wortelen der woorden betreft' (1858: 382–3). ('Although the Ainu language has refined itself through intimate contact with a civilized people, it has none the less retained its original character and distinguishes itself as a singular and independent language, which bears no similarity to any of the languages of neighbouring regions in terms of its lexical roots.')

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3 *Archaeology, linguistics and the expansion of the East and Southeast Asian Neolithic*

CHARLES F.W. HIGHAM

INTRODUCTION

The conjunction of archaeological and linguistic data has a long ancestry in Southeast Asia, where the first recorded instance of comparative linguistics in the region occurred in 1603. De Houtman, a Dutch sea captain, noted similarities between the Malay and Malagasy languages. De la Loubère (1693) made the first recorded comment on the origins of the Thai based on linguistic evidence when he wrote:

> As for what concerns the origine of the Siameses, it would be difficult to judge whether they are a single people, directly descended from the first men that inhabited the contrey of Siam, or whether in the process of time some other nation has not also settled there, notwithstanding the first inhabitants. The principal reason of this doubt proceeds from the Siameses understanding of two languages, viz. the vulgar, which is a simple tongue consisting almost wholly of monosyllables, without conjugation or declension, and another language, which I have already spoken of, which to them is a dead tongue known only to the learned, which is called the Balie tongue, and which is enricht with the inflexions of words, like the languages we have in Europe.
>
> (de la Loubère 1693: 14)

De la Loubère was referring to Thai, a member of the Tai-Kadai (or Daic) family, and to Pali, an Indo-European language. Had he travelled more widely outside Ayutthaya, he would also have encountered communities speaking Mon, an Austroasiatic language, Cham, an Austronesian language and Karen or Chinese, both of which are Sino-Tibetan. Clearly, the linguistic history of Southeast Asia is complex.

Since then, much research has been undertaken on the languages of Southeast Asia and information relevant to any consideration of the area's prehistory has been obtained. Yet, there is much still to be done. Some

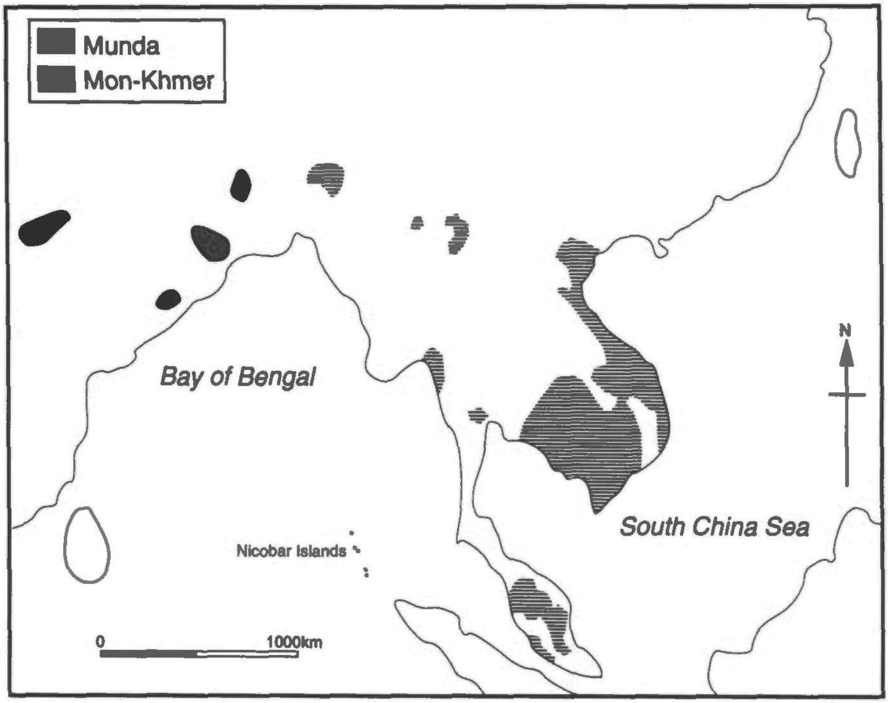

Figure 3.1 Distribution of the Austroasiatic languages in Southeast Asia

languages remain names on a map, and the pace of change threatens extinctions and the loss of critical information. We must also be cautious in attempting to relate the present distribution of languages to the archaeological record. Languages can become extinct, words can be adopted across considerable distances without population movement and the pace of linguistic change can be highly variable.

THE LANGUAGES OF SOUTHEAST ASIA

The distribution of languages in mainland Southeast Asia is so kaleidoscopic that seeking meaningful patterns is only for the foolhardy or courageous (Figures 3.1–3.3). There are five major language phyla: Sino-Tibetan, Austronesian, Daic (=Tai-Kadai), Austroasiatic and Hmong-Mien (=Miao-Yao). Is it possible to identify a possible relationship between one or more of these families with a neolithic expansion? One basic problem is that, according to recent excavations in the middle stretches of the Yangzi Valley, the transition to rice cultivation there began over 8,000 years ago. If there was a subsequent diaspora of agriculturalists, the language or languages spoken then could have developed along such different paths in the many areas

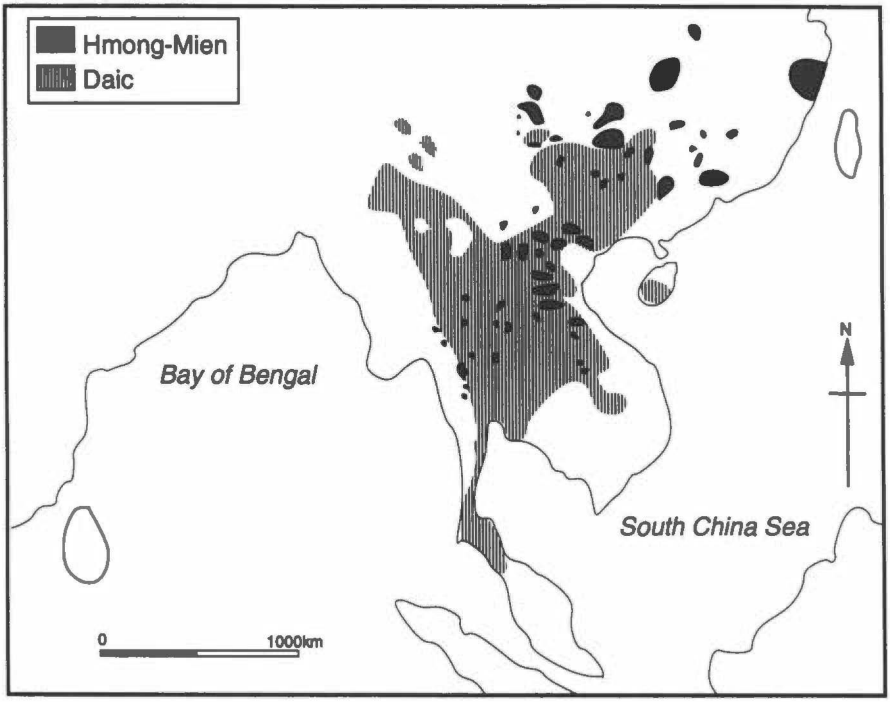

Figure 3.2 Distribution of the Tai, Kadai and Hmong-Mien languages in Southeast Asia

settled, that few if any similarities would survive. Take, for example, Maori, one of the most remote of the Austronesian languages. Its relationships with Munda languages of eastern India would be hard to find, although they may well share a common ancestor. There is a second problem. During the early phases of expansion, let us say from 6000 BC to 2000 BC, extensive lowland tracts of Southeast Asia were progressively covered by the sea. So, sites belonging to this crucial time-span are lost. Could there have been a transition to rice cultivation where there is now a shallow sea?

Sino-Tibetan (or Tibeto-Burman) and its distribution is exhaustively treated by van Driem (Ch. 2, this volume). Chinese imperial expansion into Lingnan during the late first millennium BC involved intimate contact with people who spoke Tai-Kadai and Austroasiatic languages. The latter comprises over 150 languages within two mainland sub-families: Munda and Mon-Khmer (Diffloth 1991). Vietnamese and Khmer are the best known of the latter, but less is known of a further member, Mon, formerly widely spoken in Central Thailand. However, with the southern expansion of Thai speakers during the past millennium, Mon now survives only in pockets on the margins of the Chao Phraya valley. Surviving inscriptions from Central Thailand reveal that Mon was the language of the Dvaravati Civilization of the first millennium

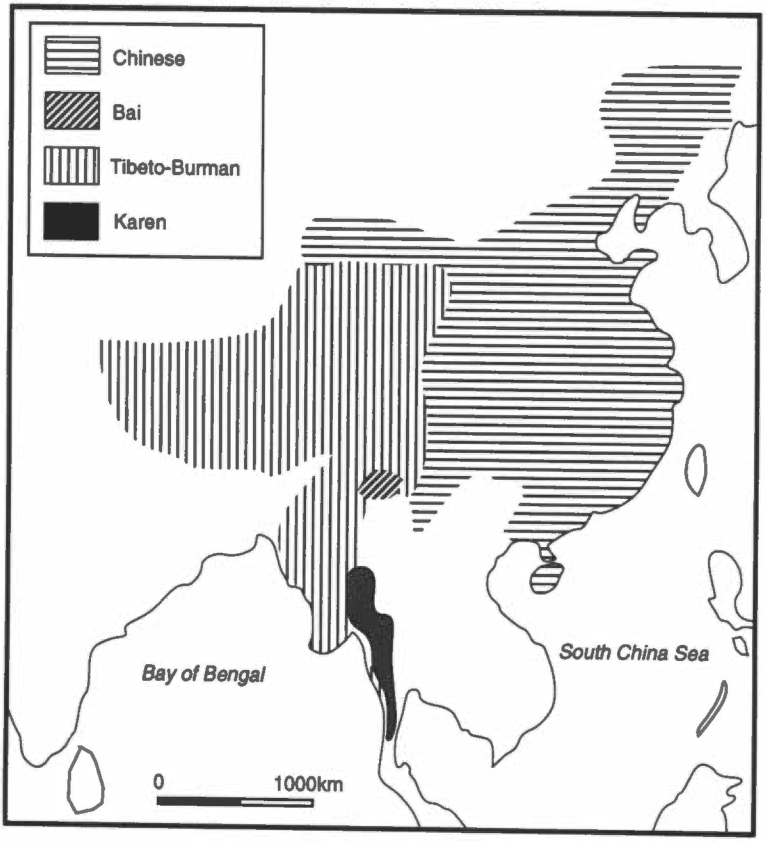

Figure 3.3 Distribution of the Sino-Tibetan languages in Southeast Asia

AD, and most workers agree that this group had local roots. Khmer is the national language of Cambodia and again, the earliest inscriptions in the Lower Mekong Valley include old Khmer texts. Vietnamese speakers have expanded during the last millennium from the Red River Valley to coastal Central Vietnam and the Mekong Delta region. This southward spread has led to a considerable reduction in the area where Cham is spoken. The Chamic languages are Austronesian with close similarities to languages of Island Southeast Asia. Chamic was probably introduced into Central Vietnam during the first millennium BC from Island Southeast Asia.

This geographic fragmentation of Austroasiatic languages is seen elsewhere in what was surely once a broad belt of people belonging to this language family. A second sub-family is found in Central and Eastern India, where it is spoken by about 5 million Munda people. They are rice cultivators whose distribution is thought to have been reduced by pressure from other Indian groups. While stoutly retaining their identity, they live in relatively remote areas.

The widespread distribution of Austroasiatic languages must be considered with their considerable diversity. Even Mon-Khmer has twelve main branches, and Diffloth has suggested that their differences are compatible with separation as long as 3,000–4,000 years ago. At the same time, groups belonging to the Austroasiatic language family have been exposed to many intrusions. We have seen how Thai, Hindi and Karen speakers have cut up the distribution of Austroasiatic into isolated groups. The Chinese have also had a major impact, particularly in Vietnam, for the northern part of that country was part of the Chinese empire for eight centuries.

At various times, linguists have proposed genetic links between the language families of Southeast Asia. Schmidt (1906) linked Austronesian and Austroasiatic. Benedict (1942) joined Austronesian, Miao-Yao and Tai-Kadai as Austro-Tai, which would *ipso facto* link Miao-Yao and Tai-Kadai with Austroasiatic. Sagart (1994) has suggested that Old Chinese (Sino-Tibetan) and Austronesian might share a common ancestor. Thurgood (1994) has pointed to evidence for contact between Tai-Kadai and Austronesian, but does not confirm Benedict's hypothesis of a genetic relationship. Assessing these various linkages is for linguists: I have no inclination to step on thin ice. My only intention in this chapter is to explore the implications of one proposed genetic relationship which has recently received support from three powerful advocates: the confirmation of Schmidt's Austric hypothesis, which sees a common ancestry for Austroasiatic and Austronesian.

THE AUSTRIC HYPOTHESIS AND NEOLITHIC EXPANSION

This development, which has major implications for the prehistorian, has been based on the Austroasiatic Nancowry language of the remote Nicobar Islands. Reid (1994) has stressed morphological relationships between Nancowry and Austronesian. To this, Diffloth (1994) has added a number of possible cognates between Austronesian and Austroasiatic. Blust (1996) was quick to identify a possible relationship between the present distribution of Austroasiatic languages and the spread of agriculturalists from the area of the Upper Yangzi. The distinction between the Munda and Mon-Khmer branches of the Austroasiatic family is deep, and reveals an early divergence (Pejros and Shnirelman, Ch. 16, this volume). Blust suggested that the expansion of early Austroasiatic speakers followed the line of least resistance, the rivers. So the Brahmaputra was followed by speakers of early Munda languages, while the Red, Mekong, Chao Phraya and Chindwin-Salween-Irrawady were followed by speakers of languages which gave rise to Vietnamese, Khmer and Mon (Figure 3.4).

A common origin of Austroasiatic and Austronesian languages in the Austric phylum necessarily places pre-Austronesian languages on the Chinese mainland, and probably again, in the Yangzi Valley. Blust (1985) has argued that the oldest Austronesian languages are to be found on Taiwan. The expansion

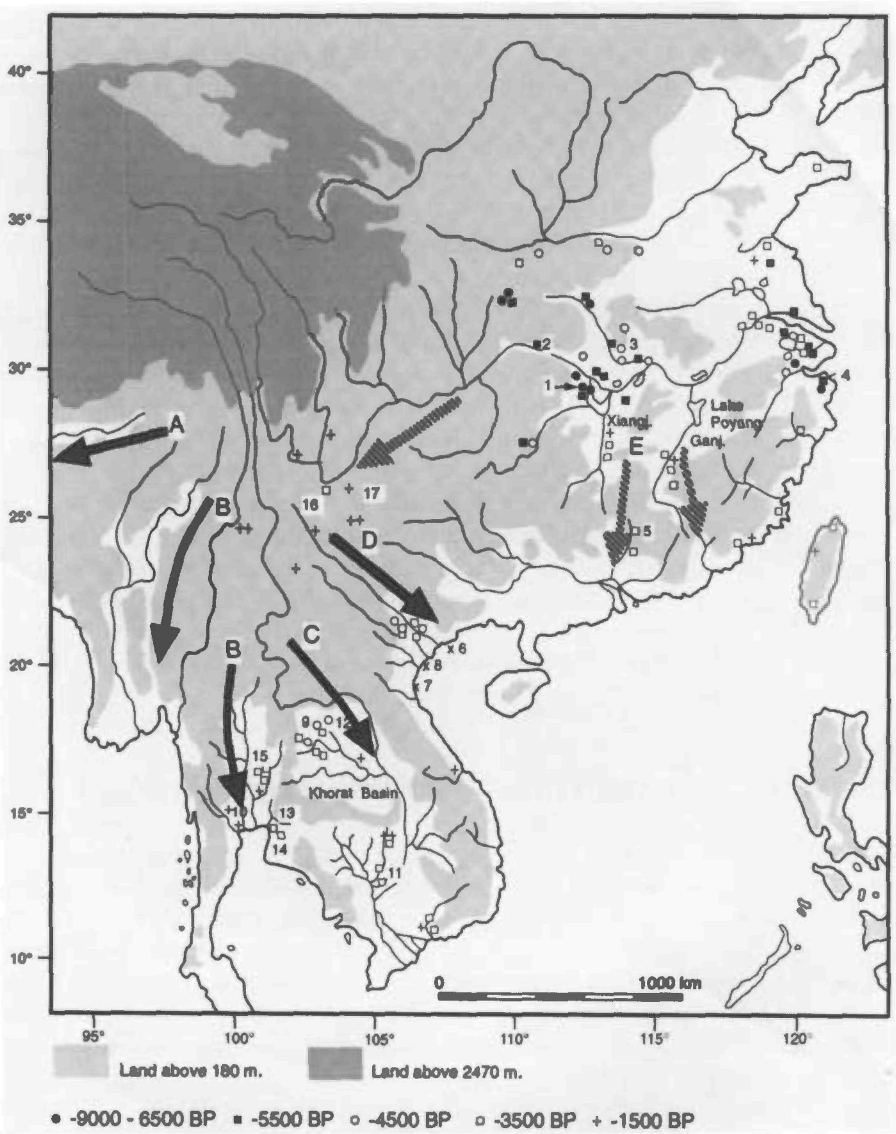

Figure 3.4 Sites mentioned in the text, and possible expansionary routes for speakers of early Austroasiatic languages

1 Pengtoushan 2 Daxi 3 Qujialing 4 Hemuda 5 Shixia 6 Cai Beo 7 Quynh Van 8 Da But 9 Non Nok Tha 10 Ban Kao 11 Samrong Sen 12 Ban Chiang 13 Khok Phanom Di 14 Nong Nor 15 Non Pa Wai 16 Baiyangcun 17 Dadunzi A Proto-Munda B Proto-Mon C Proto-Khmer D Proto-Viet E Proto-AA

Black arrow: probable expansion of AA speakers

Shaded arrow: suggested expansion from source in Yangzi Valley of early AA speakers

south to the Philippines and beyond has been described by Bellwood (1991, 1992, 1993) and Spriggs (1989, Ch. 4, this volume).

How does the archaeological evidence fit this proposed expansion? There are two different but not incompatible possibilities. One sees a major wave of advance of agriculturalists, originating in the Yangzi Valley, infiltrating tropical Southeast Asia and eastern India from the third millennium BC. The other, supported by Meacham, stresses the likelihood of indigenous origins in southern China for the neolithic communities there (Meacham 1983, 1985, 1991).

In terms purely of our knowledge of the archaeological record and the present distribution of languages, it is hard to avoid the conclusion that the early rice-farming communities of the Yangzi Valley, identified from Pengtoushan to Hemudu, spoke languages within the Austric phylum, one of which was ancestral to Austroasiatic. The distribution of Austroasiatic languages is compatible with a mainland riverine spread in a southerly direction from an original centre of rice cultivation in the extensive marshlands of the Yangzi Valley. Sites which have yielded rice remains are later as one proceeds in a southerly direction. There is also a proliferation of sites in the Yangzi Valley itself. In the middle reaches, we find that the early site of Pengtoushan was succeeded by numerous settlements of the Daxi culture (4500–3000 BC), which in turn developed into the Qujialing culture (3000–2500 BC: Chang 1986). This extended as far east as Lake Poyang, and the sites have in common large samples of rice, the domestic pig and dog, clay spindle whorls and cemeteries in which the dead were interred with offerings which included pottery vessels of widespread forms: the *ting* tripods and pedestalled bowls.

Two rivers provide communication through the southern uplands in the direction of Lingnan and the coast of the South China Sea, the Xiangjiang and the Ganjiang. At the confluence of the latter with the Yangzi, we encounter similar rice-cultivating village communities belonging to the Shanbei culture, dated to the third millennium BC. Chang (1986) has acknowledged the widespread distribution of similar pottery and other artefact forms in the Shanbei and many other regional groupings at this juncture by ascribing them to the Lungshanoid horizon. A most intriguing question is the southernmost limit of settlements which may belong to it. Chang is in no doubt, for example, of the Lungshanoid affiliations of Shixia on the Beijiang River (*c.* 2800 BC). This settlement contained not only a large sample of rice, including offerings in some of the 108 graves excavated, but also the Lungshanoid-style tripods and jade ornaments in the form of rings, beads and pendants. The assemblage matches very closely those recovered in the Shanbei culture to the north, and along with the nearby site of Niling, is most easily explained as a southward, riverine expansion from the mid-Yangzi area.

Although Austroasiatic languages are notable for their absence in Lingnan, there is a corpus of words which might represent an Austroasiatic substrate. Norman (1985) for example, has considered the Chinese time-cycle, which incorporates a cycle of twelve earthly branches and ten heavenly stems. This

was used at the beginning of Chinese history, and the symbols are among the most frequently found graphemes on the Shang oracle bones. Norman has found that the names for six of the graphemes used to describe animals have an Austroasiatic origin, the Chinese forms most closely resembling those in Vietnamese and Muong.

Norman and Mei (1976) have argued that Austroasiatic languages were formerly spoken well into what is now southern China on the basis of loanwords into Chinese. Thus the word for 'to die' in eastern Han China has an Austroasiatic origin. Old Chinese words for ivory and tiger and even the word *chiang* for the Yangzi River are said to be Austroasiatic. They have also shown that the Austroasiatic word for dog, present from Assam to Vietnam, was in use in Vietnam during the second century AD. The Min dialect is spoken in Fujian and northeastern Guangdong, provinces on the southeast coast of China. While most words can be traced back to early Chinese, there are some, such as the words for shaman, child, son, crab and small fish, which have an Austroasiatic origin. Norman and Mei see these as evidence for an early Austroasiatic substratum there. Hashimoto (1972) has also studied the languages of southeastern coastal China, and has found words in Cantonese and Min which indicate that Austroasiatic languages were once spoken in that region.

The archaeological evidence for the putative intrusive movement into Southeast Asia comprises a series of small villages which incorporate rice cultivation, the domestic dog and inhumation cemeteries. These are widely dated to the later third millennium BC. In Vietnam, the Phung Nguyen culture sites cluster above the confluence of the Red and Black Rivers. Ha Van Tan (1985) has pointed out that the preferred method of decorating Phung Nguyen pottery involves incised curvilinear bands infilled with comb or shell impressions. He has pointed to parallels with pottery from Samrong Sen in Cambodia and Ban Kao and Non Nok Tha in Thailand. We can now also add to this list, the early incised pottery of Non Pa Wai and Ban Chiang, and possibly some of the mortuary wares from Khok Phanom Di.

In Yunnan, there are further sites which incorporate inhumation cemeteries with dentate stamped and burnished pottery. Baiyangcun is located only 60 km east of a tributary of the Lancang (Upper Mekong) River, close to the headwaters of the Red River. Excavations have revealed a stratigraphic sequence 4.35 m deep, divided into two phases (Yunnan Provincial Museum 1981). The foundations of eleven houses have been identified, and a cemetery of at least thirty-four burials. The latter were orientated with heads to the north or the east. Their layout suggests the presence of two clusters, in which there is at least one double burial. Unusually, there are no grave goods, and many of the skeletons lack a cranium. The pottery was decorated with parallel incised lines infilled with impressions, a technique with parallels in Phung Nguyen and many other sites to the south, and the single radiocarbon date of 3770 ± 85 BP (2462–2014 BC at 2 sigma) indicates contemporaneity with early Phung Nguyen and Non Pa Wai. The closest parallels to the pottery

decoration strongly suggest links with communities down the Red and Mekong Rivers.

The same may be said of Dadunzi (Yunnan Provincial Museum 1977). This settlement covers 0.5 ha, and excavations in an area of nearly 500 square metres have revealed fifteen house plans and twenty-seven burials in the cemetery. Houses were orientated on a north–south or an east–west axis, and superpositions indicate some length of settlement, the subsistence base of which included rice cultivation and the raising of domestic stock. Adults were buried in an extended position and infants were interred in jars. There was no preferred grave orientation, and the infant jar burials were not regularly placed in association with adult burials. Once again, the pottery was decorated with infilled incised bands and the single radiocarbon date of 3210±90 BP (1549–1414 BC) falls within the chronological range for this tradition to the south.

It is, therefore, suggested that the widespread distribution of dentate stamped pottery, new configurations of inland settlements of rice cultivators and inhumation burial rites in permanent cemeteries could reflect a neolithic wave of advance into Southeast Asia via the main river systems. According to present evidence, this took place during the third millennium BC, but many sites were not occupied until the second millennium and large lowland tracts, such as the Khorat Basin, remained without human settlement into the first millennium.

Meacham (1983, 1991), however, has stressed the importance of the drowned land which would once have stretched up to 100 miles out across what is now the South China Sea. This area of tide-dominated deltas, mudflats and mangrove swamps could have sustained a population of sedentary communities which lived off the rich, self-replenishing marine resources. Meacham has named this drowned country Nanhailand. It is highly significant to find that, as soon as the sea stabilized at a level slightly higher than it is at present, settlements were established all the way from Taiwan to central Vietnam. These date back six millennia, and the people in question lived off fish and shellfish, made pottery and polished stone adzes, used sandstone polishers, and, on occasion, buried their dead within their settlements. At Tung Shan, for example, located in Hong Kong, the lowest level contained the remains of sand-tempered, cord-marked pottery and flaked stone tools (Qiao Xiaoqin 1991).

Similar coastal sites are known along the coast of Vietnam and the Gulf of Siam. Settlements were established soon after the formation of the new, raised coastline. They pose issues of considerable importance in Southeast Asian prehistory. Do they represent maritime communities responding to the rising sea level by relocating their settlements? This seems the most likely explanation for their number and the variety of material culture encountered. There is no evidence either in Vietnam or in southern China for a migration of people from anywhere else. While resolution of this issue may have to await the discovery of sites under the present shallow sea which overlies the

continental shelf, Ha Van Tan (1985) has stressed that the stone-working tradition seen at Cai Beo, Da But and Quynh Van culture sites match closely that found in the inland Hoabinhian and Bacsonian rockshelters. Since these latter sites were occupied, with no room for doubt, for at least six millennia before the earliest of these coastal sites, there are strong grounds for proposing a long-term continuity in the settlement of this part of Southeast Asia over the past 10,000 years at least. This could be extended back another eight millennia if we include the Son Vi culture. We also find that the coastal sites made use of netweights, became steadily more proficient at grinding and polishing stone tools, and interred their dead in inhumation cemeteries.

The Vietnamese call these sites neolithic, but this raises an issue in need of close examination. No evidence has yet been found for the cultivation of rice in these early coastal sites. This may well be the result of insufficient sampling, for sophisticated retrieval techniques must be employed, particularly where rice chaff was not used as a tempering agent in pottery, to recover fragile plant remains. We cannot, however, doubt that there was a dense distribution of these sedentary, coastal groups in southern China and Vietnam.

A TEST CASE

In microcosm, the two scenarios outlined have been tested on former shorelines behind the present Gulf of Siam. Two sites have been examined in the valley of the Bang Pakong River. Nong Nor is a small occupation site formerly located on the edge of an extensive marine embayment, and dated to about 2450 BC. Khok Phanom Di, 14 km to the north, is a much larger settlement and cemetery, dated between 2000 BC, and 1500 BC, and situated in an estuarine habitat (Higham and Thosarat 1994). The former has provided no evidence for rice or the domestic dog. The latter has abundant, cultivated rice and the domestic dog, which is thought to be descended ultimately from the Chinese wolf. The first overview outlined above would see the latter as representing an intrusive neolithic group infiltrating a coastal tract occupied by affluent foragers. But the material culture, including the incised pottery decoration, pot forms, adze types and bone artefacts are unanimous in linking the two (O'Reilly 1997). It must, therefore, be noted that the situation was probably complex as indigenous coastal groups were exposed to an intrusive neolithic. There may have been a local transition to rice cultivation, or a degree of acculturation in the face of expansive settlement.

One key to confirming or rejecting the hypothesis that Austroasiatic languages and rice cultivation were introduced into Southeast Asia lies in the examination of human remains. For example, the new means of amplifying prehistoric mtDNA may in due course make it possible to identify links between the people interred in Chinese and Southeast Asian cemeteries.

CONCLUSION

The linking of Austroasiatic and Austronesian into an Austric phylum has opened the possibility that the expansion of Austronesian speakers over much of Island Southeast Asia and beyond originated on the Asian mainland. If both share a common origin, then we can also identify a parallel mainland expansion of Austroasiatic speakers into tropical Southeast Asia and eastern India. There is some archaeological evidence for this mainland expansion, since rice-cultivating communities settled the lowland areas at least by the later third millennium BC. Much archaeological research is necessary before this neolithic expansion can be accepted, still less seen as the only means whereby rice cultivation came to be established. The tropical shore of Southeast Asia was occupied by numerous at least semi-sedentary communities who made pottery and ground stone implements, and buried their dead in inhumation graves. There are several alternatives, but they are not mutually exclusive. A neolithic expansion into tropical Southeast Asia, bringing early Austroasiatic languages, is one. A local coastal transition to rice cultivation may have occurred. Again, exposure to intrusive communities which cultivated rice and brought domestic dogs could have exposed local coastal groups to agriculture and encouraged its adoption. The establishment of inland agricultural villages in the third millennium BC from eastern India to Lingnan and Vietnam is seen as the archaeological correlate of a wave of advance. But a detailed review of evidence from the Gulf of Siam area also suggests the local adoption of rice cultivation by indigenous hunter-foragers. Of one fact, however, there can be no argument: the contribution of linguistic evidence for Austric has opened a new vista on our understanding of the archaeological record.

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4 *From Taiwan to the Tuamotus: absolute dating of Austronesian language spread and major sub-groups*

MATTHEW SPRIGGS

INTRODUCTION

The methods of historical linguistics can suggest likely places of 'origin' (in reality meaning only the earliest inferrable stage) of a language group, and the geography of its spread from such a point of origin. Earlier stages of the language can be reconstructed with associated vocabularies and likely cultural associations. What historical linguistics on its own cannot convincingly achieve is a chronology for the spread of a language group or for the dating of a particular language stage or proto-language. Only if a convincing connection can be made between language distribution and distribution of an archaeological horizon or cultural complex can the chronology of spread of a language group be postulated.

Archaeologists reconstruct sequences of change in cultures through time, and can date them, but their evidence is of course of a very different kind from that used by linguists. At one level there is no relation between the two sets of sequences produced by these very different disciplines, and yet as Ross points out in his chapter on the Central Papuan Coast (Ch. 6, this volume), both kinds of sequences are manifestations of change in human societies. Major linguistic events such as the spread of related languages through an area are likely to be reflections of processes which have archaeological correlates. Archaeological evidence for the progressive spread of an agricultural society with a unitary culture through a previously non-agricultural region would suggest an actual migration of farmers was involved. If this were to be the hypothesis, then there would certainly be expected linguistic correlates of such a process (cf. Bellwood 1991).

In the Island Southeast Asian region we do have archaeological evidence for such a spread of an agricultural (in conventional terms 'Neolithic') culture starting about 5,500 years ago (Bellwood 1985). Its eastern extension into the Pacific is identified with the Lapita Culture and various successor cultures in Near and Remote Oceania. Linguists have provided evidence for the spread of the Austronesian language family across the same vast area (Pawley and

Ross 1993). This has its origins in Taiwan, the same area as that of the immediate origins of the Island Southeast Asian Neolithic spread. That two such major cultural phenomena, established using independent data of very different kinds, are found to have occurred in the same geographic range can hardly be coincidental.

THE LAPITA CULTURAL COMPLEX AND ITS ISLAND SOUTHEAST ASIAN ORIGINS

The Lapita culture is known for its elaborately decorated pottery, much of the decoration being executed by a series of small toothed or 'dentate' stamps (Green 1990). Sites with this pottery occur from New Guinea in the west to Tonga and Samoa in the east, in a horizon beginning 3500–3000 BP (Figure 4.1). Beyond the main Solomon Islands, in Remote Oceania (Vanuatu, New Caledonia, Fiji and Western Polynesia) Lapita represents the culture of the initial human colonists. Lapita is not just pots. There is a whole 'package' of material culture items such as stone and shell tools and ornament types and other distinctive features such as permanent village settlements which, like the pottery, are not found in earlier cultures of the Near Oceania 'homeland' area (Spriggs 1993, 1997).

In a series of books and papers, Peter Bellwood (in most detail in Bellwood 1985) has presented the Southeast Asian archaeological evidence which demonstrates that the Lapita Culture derives directly from the Island Southeast Asian Neolithic. This in turn can be traced earliest in Taiwan. Bellwood also postulated an ultimate origin of the parent Taiwanese cultures in South China, work extended by Tsang (1992) who has usefully summarized some of the recent research on the Chinese mainland. This is not the topic of this chapter, but as summarized by Tsang there is a series of corded-ware sites on the Fukien and Kwangtung coasts dating to the period 7000 to 5000 BP comparable in detail to the earliest Neolithic Ta-pen-k'eng culture sites of Taiwan. The corded-ware horizon in South China goes back even earlier at a series of sites further inland in Kwangtung, Kwangsi and Kiangsi dating between about 10,000 and 7000 BP.

Lapita elements of Southeast Asian origin would appear to include pottery (or at least particular kinds of pottery), domestic animals, quadrangular adzes, polished stone chisels, various shell ornament types, rectangular houses (some on stilts), large villages, tattoo chisels, pearlshell knives, trolling hooks and various stone artefact classes. Elements which were widespread pre-Lapita in Southeast Asia and Melanesia and are therefore unassignable as integrations or intrusions in this culture include: oval/lenticular polished adzes, grindstones, hinge-region *Tridacna* adzes, pierced shell pendants, shell beads, *Trochus* armbands, one-piece fishhooks, bone points or awls, vegetation clearance by fire, movement of wild animals/plants, most of the Oceanic domesticated crop complex, shell fishing and reef fishing, possibly earth ovens, and some

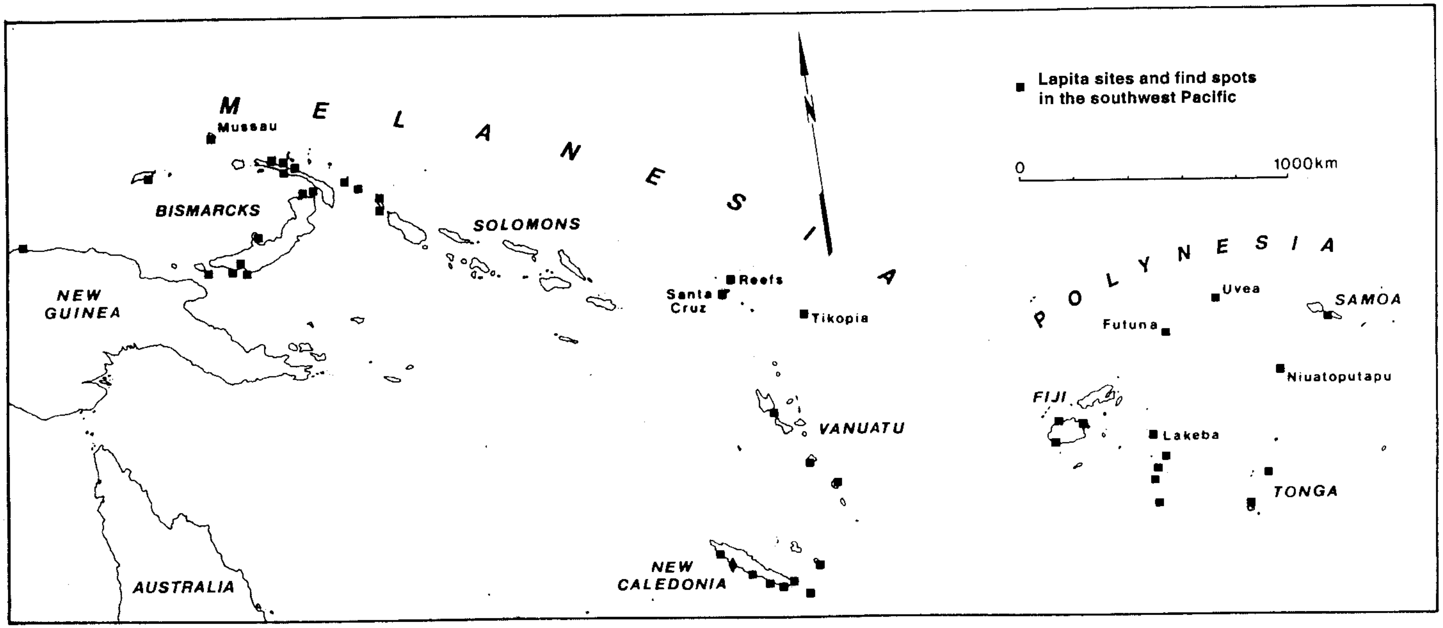

Figure 4.1 Lapita sites and findspots in the Southwest Pacific

level of long-distance exchange. Unique to Melanesia on current evidence (integrated somewhere between Cenderawasih Bay and the Bismarcks) were very few elements: some crops, obsidian stemmed tools and dorsal-region *Tridacna* adzes and perhaps the earth oven (Spriggs 1997).

Starting with a paper in 1989 I have been examining the dating of the spread of pottery-using, agricultural cultures through Island Southeast Asia and Near Oceania, and their extension, ultimately aceramic, into Remote Oceania. Four papers, one co-authored with Atholl Anderson, have sought to provide a critical evaluation of the regional radiocarbon corpus and its interpretation (Spriggs 1989, 1990, 1996a; Spriggs and Anderson 1993). The chronological pattern tentatively identified in Island Southeast Asia and more stridently asserted for Island Melanesia and Polynesia is of a west to east cline in the dates for the beginning of the Island Southeast Asian Neolithic and of the Lapita culture, its eastward extension into Near and Remote Oceania. My conclusion in the 1989 paper was that

> a fairly homogeneous Neolithic culture (with some regional variation to be sure) was established in Taiwan by 5500 BP, in Luzon and Sulawesi at 5000 BP, in Timor by 4100 BP, and in the Bismarck Archipelago by 3900–3500 BP. . . . Set down in a settlement on Taiwan, Timor, Manus and perhaps even Tonga in 3000 BP one would find oneself in the same cultural milieu. All that changed after about 2000 BP. The cultures of the Island Pacific and Island Southeast Asia diverged rapidly after that time and their later archaeologies look very different
>
> (Spriggs 1989: 608)

Subsequent analysis shows that this conclusion on dating and early cultural homogeneity still stands, although the earliest firm evidence for Lapita in the Bismarck Archipelago is better placed at 3500 BP (Spriggs 1996a). First settlement in Western Micronesia, in the Mariana Islands, occurs about 3500–3000 BP (Craib 1993). The culture is derived from the Island Southeast Asian Neolithic and there are some direct parallels in pottery decoration with the Philippines. Firm evidence for human colonization of the rest of Micronesia begins about 2000 BP, with pottery-using cultures which could have been derived from a wide area of Island Melanesia, from the Admiralties to Vanuatu (Intoh 1992). The first evidence of Lapita sites outside the Bismarcks dates to about 3200 BP for the Solomons, Vanuatu and New Caledonia, and 3000 BP for Fiji and Western Polynesia (Spriggs 1990).

Here the Lapita trail goes cold. In Eastern Polynesia there have been only very occasional finds of plain pottery (ultimately Lapita-derived) in the Cook Islands and the Marquesas. The archaeological record in general is clearly of a founding culture derived from a Lapita or immediately post-Lapita base in Western Polynesia (Kirch 1984: Ch. 4). Spriggs and Anderson's (1993) analysis of radiocarbon dates from Eastern Polynesia suggests permanent occupation of the Marquesas somewhere in the period 1600–1300 BP, Easter Island

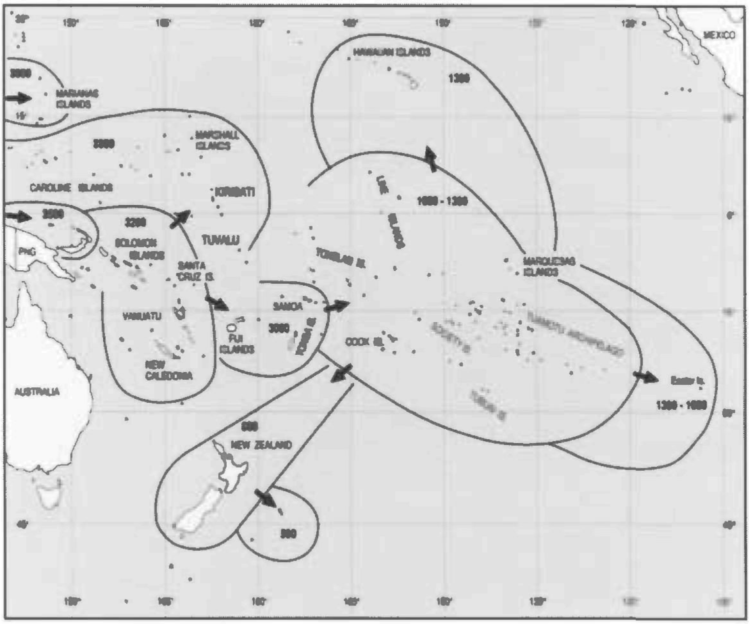

Figure 4.2 Dating of the spread of human settlement into the Pacific (short chronology)
Source: after Spriggs and Anderson 1993

sometime after 1550 BP, Tahiti at about 1300 BP, Hawaii after 1300 BP, the Southern Cook Islands at about 1000 BP and New Zealand between 950 and 750 BP (Figure 4.2). Thus, a long pause of at least 1,300 years can be identified between settlement of Western Polynesia about 3000 BP and the earliest archaeological sites further east.

Some have suggested that this pause is merely a sampling problem and that much earlier sites remain to be found. Irwin (1980, 1992) has postulated a continuous settlement model with suggested rates of discovery of the ever more remote island groups which would provide earlier dates. Thus, he would expect evidence eventually to be found for settlement of the Cook Islands soon after 3000 BP, the Societies, Tuamotus and the Australs by about 2500 BP, the Marquesas, Rapa, Mangareva and Pitcairn by 2000 BP, Hawaii and Easter Island by 1500 BP and New Zealand by 1000 BP (Figure 4.3). This long chronology view has received support from Kirch and Ellison (1994; see also Ellison 1994) on the basis of pollen core sequences from Mangaia in the Cook Islands. The pollen evidence has been used to suggest settlement by about 2500 BP on Mangaia, some 1,500 years earlier than the first direct cultural evidence of occupation. Kirch and Ellison point out that this new evidence would support Irwin's predicted age for settlement of the Cooks and then extrapolate, unwisely I feel, to suggest that this thereby demonstrates earlier settlement for the rest of Eastern Polynesia than Spriggs and Anderson

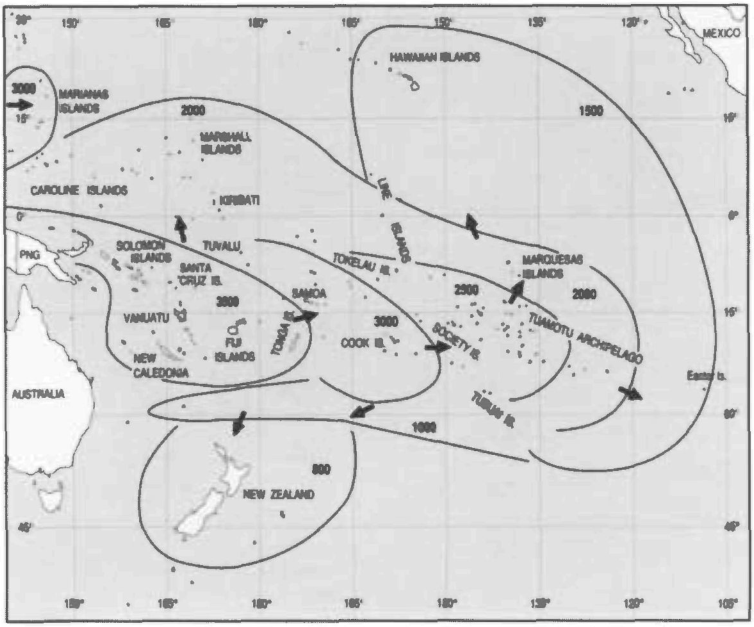

Figure 4.3 Dating of the spread of human settlement into the Pacific (long chronology)
Sources: after Irwin 1992; Kirch and Ellison 1994

have considered. Pukapuka in the Northern Cook Islands had earlier provided occupation dates, albeit somewhat problematic, of 1900–1600 BP (Spriggs and Anderson 1993: 209). That the situation may be somewhat different in the archipelago nearest the Western Polynesian source area, as foreshadowed by the Pukapuka dates, does not to my mind necessitate rejection of the rest of the Eastern Polynesian radiocarbon corpus. Indeed, as we shall see later, there is independent linguistic evidence that it should not.

The Island of New Guinea at present is almost entirely a Lapita blank. Sherds of a single (probably late) Lapita pot have been found at Aitape in the centre of the north coast but no recognizably Lapita sites have yet been found. This is more certainly a sampling problem, and there is some indirect evidence that a Lapita or 'Proto-Lapita' presence along the north coast will be found (cf. Spriggs 1996b). The Southeast Asian–Island Melanesian archaeological connection has been obscured by a large sampling gap of sites of the relevant time period in northern New Guinea and islands immediately to the west. What there is along the north coast is evidence of the spread from east to west of a probably Lapita derived pottery-using culture at about 1500 BP. More directly comparable to Lapita is the archaeologically instantaneous spread at about 2000 BP of a culture associated with the Papuan red-slipped pottery style, again from east to west, along the south coast of

the Island (see Ross, Ch. 6, this volume, for detailed discussion of the archaeology and linguistics of the area).

THE WEST TO EAST SPREAD OF THE AUSTRONESIAN LANGUAGES

A survey article by Pawley and Ross (1993) discusses the main issues in the field of Austronesian (AN) historical linguistics. In summarizing their paper here, I shall merely survey the most generally accepted linguistic sequence and location for the various proto-languages which they cover. Proto-Austronesian (PAN) appears to have been spoken in Taiwan. PAN broke up as an entity either within Taiwan (some Taiwan language groups may form first-order sub-groups of AN) or when the language spread south to the Philippines. This event led to separation between Taiwan AN languages and all other AN languages, generally called Malayo-Polynesian.

Proto-Malayo-Polynesian (PMP) was probably spoken in the Philippines and in Sulawesi further to the south. It split into Western Malayo-Polynesian (WMP) and Central/Eastern Malayo-Polynesian (CEMP) with moves to east and west from this area. Whether there was ever a unified Proto-WMP is unclear. It may be that PMP split into a number of dialects, one of which was Proto-CEMP. WMP languages may just be the ones which are not descended from this particular dialect. They include the languages of Java, Sumatra, Borneo, the Philippines and Sulawesi. At some stage one of the WMP languages (or conceivably one of the Taiwan AN languages) spread to the previously uninhabited Mariana Islands in Western Micronesia. A separate migration from Island Southeast Asia appears to have resulted in first settlement in Belau, further south in Micronesia.

The move east which led to the divergence of CEMP was probably from Sulawesi to an area in northern Maluku. The next linguistic split is when Proto-CEMP split to form Central and Eastern MP. CMP may be an entity like WMP, merely the dialects left over when EMP diverged. CMP languages are today spoken in the Lesser Sundas and Central and Southern Maluku.

The divergence of EMP occurred with another eastern move from Northern Maluku to the Cenderawasih Bay area of Western New Guinea and its offshore islands such as Biak and Japen. A subsequent move again to the east along the Northern New Guinea coast to New Britain in the Bismarck Archipelago led to the break-up of Proto-EMP into South Halmahera/West New Guinea (the stay at homes) and Oceanic (OC). Possible traces of such a spread are found in loans in some North New Guinea Non Austronesian languages from a language or languages of a stage intermediate between EMP and OC (Malcolm Ross pers. comm.).

According to Ross (1989), Proto-Oceanic (POC) was probably spoken on the north coast of New Britain, in an area including the Willaumez Peninsula. He suggests a split of POC into an Admiralties group and another ancestral

to the languages of the Southeast Solomons and all those further out into the Pacific in Vanuatu, New Caledonia, Fiji and Polynesia. The St Matthias Group, north of New Ireland, might form another primary sub-group of OC. The stay at homes in the OC case continued to develop as a dialect chain that became the Western Oceanic (WOC) group. They were subsequently responsible for east to west language spreads along the north and south coasts of the Island of New Guinea, and inland along the Markham Valley of Central New Guinea (the North New Guinea and Papuan Tip groups). To the east and south, they appear to have replaced languages dating from the initial OC dispersal in New Ireland and the Western Solomons (the Meso-Melanesian group).

It has been postulated that Southeast Solomonic forms an Eastern OC group with some other sub-groups such as Central/North Vanuatu, Central Pacific, Southern Vanuatu and New Caledonia/Loyalties. This would make logical sense of the pattern of spread of these AN languages, but the evidence so far for such a group is weak. The Micronesian languages except for Chamorro (Mariana Islands), Belauan and (possibly) Yapese are also OC. Their relationship to other languages of this group is unclear. They split off somewhere between the break-up of POC and that of Proto Central Pacific (PCP). The centre of diversity of these Nuclear Micronesian languages (and therefore probably where they spread from within Micronesia) is in the eastern part of their range in Kiribati, Kosrae, Pohnpei and the Marshalls.

PCP is the ancestor of all Fijian and Polynesian (PN) languages, but was probably a dialect chain rather than a homogenous language. It has been suggested that the initial split of PCP was between Western Fijian and Eastern Fijian/PN, with Eastern Fiji only later coming under Western Fijian linguistic influence. Proto-PN split into two branches, Tongan with Niuean and Nuclear Polynesian comprising all the others and initially located in the Samoa area (see Marck, Volume III, for a detailed discussion of Polynesian).

The great quantity and nature of the linguistic innovations defining the PN sub-group compared to other sub-groups of AN suggests a long period of unified development. The difficulty of establishing any well-defined centre of diversity of OC Austronesian languages suggests a rapid dispersal of OC speakers out of the Bismarcks as far as the Central Pacific. If this were not the case and dispersal were a slow process, one would expect to find languages of later settled areas sub-grouping with one or more of the language groups of the Bismarcks or adjacent areas. The PN sub-group is well defined, however, suggesting a considerable time before its unity was disrupted by further dispersal to the east.

This point is discussed in detail by Pawley (1996). He also invokes glottochronological studies, but only as a back up, to suggest the length of unified development of PN to be in the order of 1,000 years or more. Unity occurred within diversity, however. Pawley suggests that what later became the two primary divisions within PN, Nuclear and Tongic, started off as northern and southern dialect regions within 'Pre-Polynesian'. The break-up of Proto-PN

occurred with the permanent settlement, from the northern dialect area, of areas remote from this Western Polynesian sphere of interaction. Such areas were too remote from the source communities in the Samoan area to keep in regular contact with them. At least two such eastwards dispersals occurred, one to the northern Cook Islands attested by the Pukapukan language and another which led to the Eastern Polynesian (EP) sub-group and discussed in detail in Marck's contribution to Volume III.

Again following Pawley (1996), EP may have started as a dialect chain extending across the Marquesas and Tahiti, and possibly including some of the Cook Islands and other Central and Eastern Polynesian Islands such as the Tuamotus. Proto-EP broke up when remote Easter Island was settled. A dialect chain remained in the rest of its region, with a boundary between a Marquesan dialect and a Tahiti-Cook Islands group. This Central-Eastern Polynesian (CEP) sub-group broke up with the settlement of Hawaii from the Marquesas and New Zealand from the other dialect group area.

What is missing from this outline so far is the possibility of language replacement, particularly on some of the smaller islands and atolls of Central and Eastern Polynesia. In the early 1970s, Biggs warned that a history reconstructed solely on the basis of current language distributions in Polynesia would be misleading given the likelihood of such language change (Biggs 1972). Pawley stressed that his reconstruction of events takes account only of successful colonization by particular language groups. Some islands may have undergone earlier unsuccessful colonizations or experienced language switch since first human settlement. This is documented, for instance, for Niuatoputapu which was populated by Samoic speakers when first contacted by Europeans in the seventeenth century. Coming under Tongan political control soon afterwards, its population is now Tongan speaking (Biggs 1972: 150). Such shifts are less likely for the major Polynesian archipelagos such as Tonga, Samoa or Tahiti (Pawley 1996).

LANGUAGE AND ARCHAEOLOGY IN THE ISLAND SOUTHEAST ASIAN-PACIFIC REGION

It does not take remarkable powers of deduction to see links between the archaeological and linguistic sequences across this vast region. Indeed a link between the west to east spread of the Lapita cultural complex from Island Melanesia to Polynesia, and the spread of AN languages in this area was first suggested in the 1970s before much of the detailed archaeological picture was available (Shutler and Marck 1975; Bellwood 1978). The idea that this spread was linked to a major dispersal of peoples out of Asia, through Southeast Asia and on into Polynesia was also explicit in these early formulations.

It is uncontroversial that the foundations of Polynesian languages and cultures go back to the eastern extension of Lapita in a region previously uninhabited. That the immediate Lapita 'Homeland' is further west in the

Bismarck Archipelago is now also well accepted. That AN languages, whether in Polynesia or the Bismarcks, are derived from even further to the west in Southeast Asia and ultimately in Taiwan is also generally agreed. It seems hard to imagine a process to get these languages from Taiwan to Island Melanesia and Polynesia that would not leave an archaeological trace, and the obvious trace in Island Melanesia and Polynesia at the appropriate time period (no later than initial settlement of Polynesia about 3000 BP) is Lapita.

The complementary distributions of the Island Southeast Asian Neolithic and the Lapita cultural complex on the one hand with a west to east progression in time, and that of the AN languages of the region with a similar west to east spread provide a powerful case for such a connection. Thus, Proto-Austronesian (PAN), probably spoken on or near Taiwan, split into a Formosan and a Malayo-Polynesian grouping around 5000 BP or earlier with a movement south to the Philippines and Sulawesi. Proto-Malayo-Polynesian (PMP) broke up with a move from Sulawesi across to northern Maluku at about 4500 BP. The next linguistic split – the break-up of Proto-Central/Eastern Malayo-Polynesian (PCEMP) – occurred with a language movement to the east. This new settlement probably centred in Cenderawasih Bay on the north coast of Western New Guinea, perhaps (there are no dated sites in the area) around 4000 BP, where Proto-Eastern Malayo-Polynesian (PEMP) was spoken.

A further spread east resulted in Austronesian and Lapita settlement in the Bismarcks by 3500 BP and the break-up of Proto-Oceanic (POC) as Lapita settlements spread south and east through the Solomons and out to Vanuatu, New Caledonia, Fiji and Western Polynesia between about 3200 and 3000 BP. The inability of linguists to find a centre for diversity of OC languages within this region is explained by this rapid spread into the previously uninhabited archipelagos of Remote Oceania. The long pause identified archaeologically between settlement of Western and Eastern Polynesia is also demanded by the linguistic evidence for the development of the PN subgroup before its break-up as settlement spread to the east. Archaeological sequences from Tuvalu and Tokelau are indicated as potentially crucial for an understanding of the settlement of Eastern Polynesia by the new linguistic model of Marck (Volume III). Archaeological research is being planned to remedy the previous archaeological neglect of this area. But what of the new Cook Islands evidence of Kirch and Ellison (1994), suggesting settlement there some 2,500 years ago?

Putting together the archaeological and linguistic evidence it is clear that colonists in the Southern Cook Islands at that time could not have been speaking a language directly ancestral to the Tahitic sub-group language currently found there. Settlement took place before the break-up of Proto-PN is likely to have occurred. Whether the evidence for forest disturbance at 2500 BP signals a permanent occupation of the island of Mangaia is unclear at present in the absence of discovery of any habitation sites on the island of the time-period in question. It is not necessarily a human artefact at all (Anderson 1994). The earliest site located so far, at about 1000 BP, would

fit with initial colonization from further east by a group ancestral to the present population. It is possible that an earlier population died out or switched languages in response to settlement from the Tahitian area at that time. Biggs's (1972) point concerning such events is thus well taken.

The Mangaia case has no necessary relevance either to the linguistic or archaeological sequences of the rest of East Polynesia. First settlement in the Marquesas and Tahiti area at about 1600–1300 BP represents the likely time-depth for the East Polynesian sub-group. This sub-group split up following settlement of Easter Island some time after 1550 BP, and the dialect chain which remained in Central-East Polynesia broke up after settlement of Hawaii some time later than 1300 BP and settlement of New Zealand in the period 950–750 BP. These dates may alter by one or two centuries as new archaeological evidence comes to hand, but are unlikely to change to the extent envisaged by Kirch and Ellison (1994) unless language shift has occurred repeatedly in every archipelago. On the balance of probability this seems unlikely.

Returning briefly to the west, the Western Oceanic sub-group and its three branches find archaeological expression in the spread of pottery-using cultures from east to west along both the north and south coasts of New Guinea in the period 2000–1500 BP, and less certainly in the post-Lapita continuities in pottery decoration in the Bismarcks and Western Solomons around 2500–1500 BP. Less certainly, in that the supposed archaeological expression occurs over a much wider area than the suggested linguistic shift of Meso-Melanesian group languages replacing languages more closely related to those of the Southeast Solomons and North-Central Vanuatu. Probably related pottery styles occur immediately post-Lapita in an area from the Admiralties to New Caledonia, and slightly later in Fiji (Spriggs 1997). This may be a case where a linguistic shift occurs within a matrix of other widespread cultural changes and has no distinctive signature in material culture.

CONCLUSION

Taken separately, archaeological and linguistic evidence provide parallel sequences relating to the spread of Neolithic cultures across Island Southeast Asia and the Pacific. When brought together, each informs the other. For the purposes of this chapter I have considered mainly one aspect, the contribution of archaeology to dating the linguistic sequence. Calibrated by this method, interesting areas are opened up for study to do with rates of language change and lexical replacement under different circumstances. Where the linguistic and archaeological sequences appear out of step, such as with the proposed date of initial settlement of Mangaia in the Cook Islands, then alternative archaeological hypotheses about population affinities and possible replacement need to be entertained. The linguistic picture for Polynesia would, as Pawley (1996) points out, favour a significant pause in Western Polynesia

before permanent settlement to the east, and so has a bearing on current archaeological debate about a long or short chronology for Eastern Polynesian settlement. Similarly, the previously unrecognized importance of Tuvalu and Tokelau in relation to East Polynesian settlement revealed by recent linguistic research is now directing archaeological attention to these previously neglected archipelagos.

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5 *The archaeology of Papuan and Austronesian prehistory in the Northern Moluccas, Eastern Indonesia*

PETER BELLWOOD

INTRODUCTION

The Northern Moluccas (Spice Islands) occupy an important geographical position between Indonesia and the Oceanic Islands of New Guinea, Melanesia, and ultimately Micronesia and Polynesia. They are occupied by

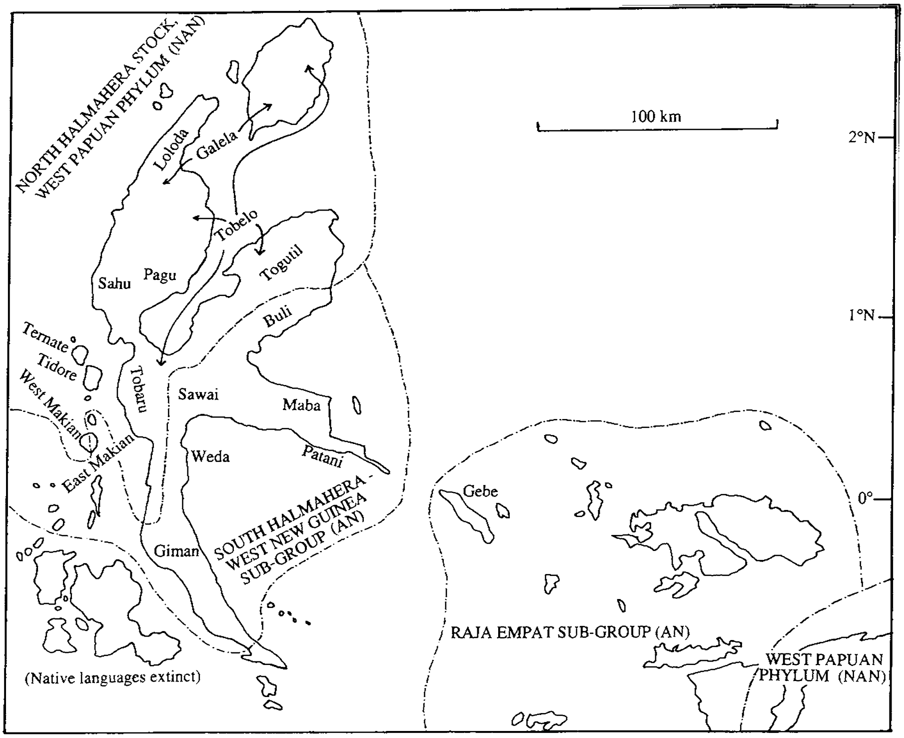

Figure 5.1 Maluku languages (AN = Austronesian; NAN = Non-Austronesian (Papuan)
Source: after Wurm and Hattori 1983: Map 45

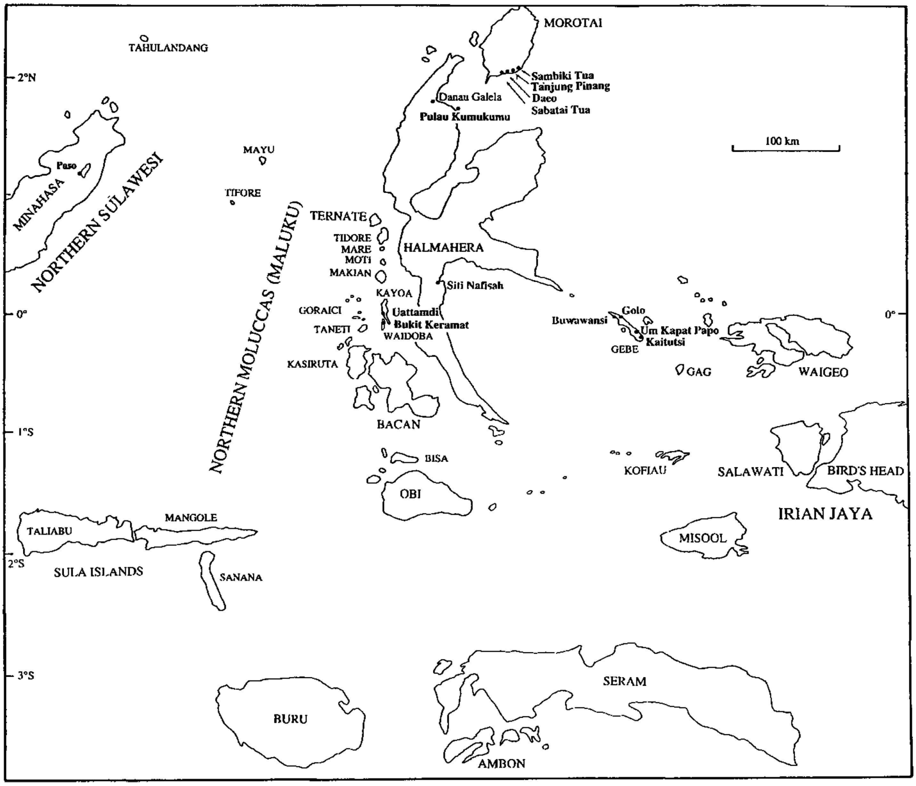

Figure 5.2 The Northern Moluccas in their regional setting

speakers of Papuan languages of presumed New Guinea derivation in the north, and by speakers of Austronesian languages unrelated to the Papuan groups in the south (Figures 5.1 and 5.2). Both populations have interacted for at least 3,500 years and this interaction has involved a great deal of intermarriage and linguistic cross-influence.

Recent and continuing archaeological research in the Northern Moluccas has produced a sequence of archaeological activity extending back at least 30,000 years.[1] A change from a pre-ceramic foraging and 'orcharding' adaptation, possibly focused on nut species such as *Canarium*, to a presumably more agricultural orientation associated with pottery, polished stone tools and introduced domestic pig and dog, is recorded on Kayoa Island at about 3,500 years ago. After 2,000 years ago the Moluccas became involved in trade networks stretching as far as India and the Mediterranean as suppliers of spices, products which also made this area attractive to the first Portuguese traders in the region after AD 1500.

The purpose of this chapter is to discuss the archaeological record of especially the past 10,000 years, focusing on variations in its content across the various surveyed regions of the Northern Moluccas. I relate this record to

linguistic understanding of the prehistory of the Papuan and Austronesian languages. This region gives a unique opportunity to study long-term inter-relationships and connections between speakers of languages within these two major groupings; no other area of Indonesia apart from West New Guinea (Irian Jaya) reflects such close-set ethnolinguistic diversity.

THEORETICAL AND MULTIDISCIPLINARY QUESTIONS CONCERNING THE GENERATION OF HUMAN DIVERSITY

In a number of papers (Bellwood 1985: ch. 7; 1991; 1994; 1996a, 1996b; 1997a) I have proposed that the overall geographical distributions of a number of major agriculturalist language families (including a major phylum discussed in this chapter – Austronesian) have resulted in large part from early colonizations by demographically expanding populations who had undergone primary or at least relatively early transitions to agriculture. These transitions occurred in regions such as Southwestern Asia, Central and Southern China, Mesoamerica, the Northern Andes, the New Guinea Highlands and perhaps Sub-Saharan Africa. Expressed succinctly, I propose colonization (not diffusion or borrowing) as the major (but not the only) process behind large-scale language family spreads and also the concurrent spreads of agriculture into territories formerly occupied by hunters and gatherers (see also Renfrew 1987, 1989, 1992). The reasoning behind this view is explained elsewhere (see above references).

This chapter is not about the macro-scale of language phyla, but about the micro-scale of linguistic, cultural and genetic interaction between geographically adjacent groups. In such cases, other processes of localized inter-group contact, apart from colonization, have a relatively greater impact on the ultimate patterning of human biological and cultural variation. In behavioural terms we witness such localized inter-group processes of interaction in the guises of technological and economic emulation (i.e. diffusion), bilingualism, language shift, intermarriage, and so forth. These are the 'rhizotic' rather than 'bifurcative' processes of ethnogenesis in the terminology used by Moore (1994). However, the intensities of these two groups of processes clearly vary according to specific historical circumstances. In the case of the Northern Moluccas we appear to be dealing with a long early phase of relative local isolation following a bifurcative phase of initial settlement, followed by two later phases of more widespread 'rhizotic' interaction. The first of these involved Austronesian immigration (clearly a bifurcative process initially) following 3500 BP, the second followed 2000 BP, after which time the islands developed an important role as the 'Spice Islands' of historical world trade. This trajectory culminated in the ultimate adoption of world religions (Islam and Christianity), and submission after AD 1450 to varying degrees of state-level authority held by the Muslim sultanates based on the twin volcano islands of Ternate and Tidore.

I want to stress again here that explanations of macro-scale distributions (e.g. of an entire language phylum such as Austronesian) can be expected to

differ fundamentally from explanations of micro-scale patterns of variation (e.g. between neighbouring Austronesian and Papuan populations in the various islands of the Northern Moluccas). Phyla such as Indo-European and Austronesian cannot have spread to any significant degree by processes such as language shift or elite dominance, even though such processes are so obviously important when we look at individual small-scale historical or anthropological situations.

The operational requirements for this chapter are to compare archaeological and linguistic patterns, both diachronic and synchronic, as *independent* sources of data about the micro-scale human past in the Northern Moluccas. Independence is here stressed since the ultimate alternative is circularity, a most undesirable research companion.

THE NORTHERN MOLUCCAS: HALMAHERA AND ADJACENT ISLANDS AND THEIR ARCHAEOLOGICAL RECORDS

The ecological key concepts for this area are equatorial climate, rainforest flora, volcanic and raised coral terrain, tectonic instability, and a location in the eastern part of Wallacea, surrounded by deep seas and never landbridged to the Sunda or Sahul continents. Existing mammal faunas are depauperate, consisting only of rats, bats and marsupial squirrel-like creatures of the genus *Phalanger* (cuscuses), but as indicated below the islands witnessed the extirpation of at least two other species of marsupials during the Holocene. In terms of Pleistocene/Early Holocene access, the easiest route for human colonizts would have been from the western end of New Guinea via the island of Gebe. Other routes from the west and south would have involved longer passages out of sight of land.

Archaeological research in the Northern Moluccas commenced in 1991. Four separate fieldwork periods comprising a total of about five months of intensive in-field research have been completed so far. The following archaeological sites have been investigated (Figure 5.2, and see Bellwood *et al.* 1993, in press):

1 Uattamdi rockshelter, Kayoa Island
2 Siti Nafisah cave at the top (eastern side) of the southern arm of Halmahera
3 Sites in southern Morotai (caves/rockshelters of Tanjung Pinang, Daeo 1 and 2, Tanjung Tulang; with open sites at Sabatai Tua and Sambiki Tua)
4 Sites on Gebe Island (caves of Golo, Wetef [excavated by Geoffrey Irwin], Kaitutsi and Um Kapat Papo, and open coastal sites at Buwawansi).

All excavated sites are essentially coastal.

A brief summary of the main points of North Moluccan prehistory is given in Table 5.1.

Table 5.1 Patterning in the archaeological and linguistic records for the Northern Moluccas, 33,000 to 500 BP

Date BP	*Morotai Island (several sites)*	*Gebe Island (several sites)*	*Gua Siti Nafisah, Weda, Halmahera*	*Gua Uattamdi, Kayoa Island*	*Economic and general cultural observations*	*Possible linguistic correlations*
33,000 to 3500 BP	Pre-ceramic occupation from 15,000 BP; volcanic pebble tool and flake industry, bone points, ochre, *Canarium* anvils. Fishing significant only in Holocene. Mammals include giant rat and cuscus, but no wallaby or bandicoot	Pre-ceramic occupation from at least 33,000 BP: core and flake industry of fine-grained crystalline rocks, ochre. After 13,000 BP: shell adzes and circular stone arrangements. After 8,000 BP: ochre-covered extended burial, bone points, *Canarium* anvils, economic focus on marsupials rather than fish. Wallaby and cuscus present, and wallaby possibly translocated from Misool Island	Pre-ceramic occupation from 5500 BP; bone points, ochre, virtually *no* stone, *Canarium* anvils, balance of marsupial and fish exploitation. Marsupials include wallaby, bandicoot and cuscus. *Rattus morotaiensis* present.	No pre-ceramic occupation reported	Observations for the Holocene (no good data for Pleistocene): *Canarium* and sago exploitation (the latter presumed only), ground-dwelling marsupials hunted on Halmahera and Gebe (not present on Morotai), fishing on Morotai and Halmahera but virtually absent on Gebe. No evidence for trade in lithic materials as yet. Cultural pattern suggests considerable local isolation.	Initial settlement by non-Austronesian speakers from Western New Guinea or Sulawest via Sula, followed by linguistic diversification.
3500 to 2000 BP	Continuation of above pattern	Continuation of above pattern	Continuation of above pattern	3500 to 2000 BP; red-slipped pottery, bone points, stone flakes, polished adzes, shell beads and bracelets, introduction of pig and dog, focus on fishing (no marsupials except cuscus present on Kayoa). This assemblage is paralleled closely from 3500 BP onwards in Philippines, Sulawesi, Sabah and in Lapita sites in Melanesia	Continuation of above with regional isolation on Morotai, Halmahera and Gebe. Introduction of domestic animals by Austronesian populations into Kayoa. Systematic field agriculture assumed but unproven for Kayoa, given PAN and PMP reconstructions for agricultural crops and technology. Uattamdi assemblage paralleled very widely in eastern Indonesia and Philippines	Austronesian settlement in Kayoa and adjacent areas, leading to beginnings of close NAN–AN linguistic interaction. NAN languages still dominate Halmahera itself.

Table 5.1 continued

Date BP	*Morotai Island (several sites)*	*Gebe Island (several sites)*	*Gua Siti Nafisah, Weda, Halmahera*	*Gua Uattamdi, Kayoa Island*	*Economic and general cultural observations*	*Possible linguistic correlations*
2000 to 1000 BP	Secondary human burials in cave floors with incised pottery, shell ornaments, iron	Incised pottery, *Canarium* anvils	Incised pottery, shell ornaments, polished stone adzes, introduction of pig and dog	Incised pottery, jar burials, bronze and iron, monochrome glass beads, Chinese coins (the latter dated *c.* 1000 BP)	International spice trade via Java and Bali to India and Western ports, Chinese interest from late first millennium AD. Pan-Indonesian ceramic pattern spread after 2200 BP with glass and metal.	Expansion of NE Halmahera Stock of PN languages, perhaps as a population dispersal? Possible spread of AN languages through eastern Halmahera, and from western New Guinea into Gebe (Gebe belongs to Raja Empat subgroup of SHWNG).
1000 to 500 BP	Increasing interaction with Ternate Sultanate	Increasing interaction with Tidore Sultanate	Increasing interaction with Ternate Sultanate	Increasing interaction with Tidore Sultanate	International trade, Islam, widespread use of Malay, warfare, considerable population disruption especially after Portuguese arrival early sixteenth century. Earliest Chinese trade ceramics enter the region	Use of Malay and continuing use of Bahasa Ternate/Tidore (both mutually intelligible) as lingua francas throughout archipelago.

First, the caves of Golo and Wetef on Gebe Island have occupation dated from *c.* 32,500 BP associated with stone tools and burnt marine shells. The Morotai Island sequence currently goes back only 15,000 years, and Morotai lacks the marsupial wallaby and bandicoot species recorded from Holocene deposits on Halmahera and Gebe. These were extirpated by 2,000 years ago on both islands (Flannery *et al.* 1995; only the wallaby has been recorded so far from Gebe). The cuscus (*Phalanger* sp.) is the only marsupial recorded from all excavated sites in the Northern Moluccas, including Morotai. There is a possibility that the wallaby was translocated to Halmahera and Gebe from populations of *Dorcopsis muelleri* located on Misool Island, off the western end of the Bird's Head of New Guinea, about 8,000 years ago (Flannery *et al.*, in preparation). The cuscus reveals no clear evidence for translocation, and the situation for the bandicoot is unknown.

Second, the pre-ceramic population, apart from collecting shellfish, seems to have lacked any major maritime orientation. Fish bones occur in Holocene layers in the Daeo 2 midden on Morotai and the Mid-Holocene Siti Nafisah cave on Halmahera, but are almost absent in the Gebe pre-ceramic layers where marsupials provided almost all of the meat eaten after their appearance about 8,000 years ago. Bone points occur in all sites, perhaps used to extract meat from small gastropod shellfish. Stone does not appear to have been traded; the Gebe and Morotai sites have many pebble and flake tools of local stone, but Siti Nafisah in southern Halmahera has only one piece of flaked stone from 3,000 years and about 3 cubic metres of occupation. Good tool-making stone, which must have had some technological value, was not apparently traded to areas where it did not occur. Although these people were somehow able to get to Halmahera and colonize in the first place, their economy was predominantly a landlubber one. Plant foods certainly included *Canarium* nuts (identified from shell fragments and distinctive stone anvils) and probably sago starch and tubers. Given the presence of agricultural practices in the New Guinea Highlands as early as 6000 BP (Golson 1977), it is quite possible that these plants were managed in the Northern Moluccas at a level beyond that of simple gathering.

Third, red-slipped (but otherwise undecorated) pottery with associated stone adzes, shell beads and bracelets, fish bones, pig and dog bones (the last two being domesticated and introduced species) appeared in the Uattamdi site on Kayoa Island at about 1500 BC. As I have indicated elsewhere (Bellwood *et al.* 1993) this assemblage relates to contemporary red-slipped pottery assemblages from eastern Indonesia and the Philippines, and also perhaps in a cousinly way to Lapita in western Melanesia. I regard it as an archaeological record of Austronesian colonization, not least because of the close fit between the archaeological record and the vocabulary reconstructed for early stages of Austronesian (Blust 1984–5, 1995; Bellwood 1997b, 1991). The interesting point is that this type of assemblage has been found only in Kayoa so far, and is quite absent from the sample recovered from the other sites and islands listed. Whether Austronesian plant food production practices were any more

complex than those of the pre-ceramic populations already resident is not known, but Austronesian linguistic reconstructions suggest that they might have been, with field agriculture of a wide range of tubers, fruits and possibly even cereals (foxtail millet, but perhaps not rice).

Fourth, on Morotai, the Halmahera 'mainland' and Gebe, pottery seems first to appear only *c.* 2000–2500 BP in the form of incised, footed, carinated and sometimes red-slipped vessels. This pottery is of a style very widespread in the Indonesian Metal Age, excavated in Bali (Ardika 1991) and in various sites in eastern Indonesia and Sabah (Bellwood and Koon 1989 [Bukit Tengkorak late phase]; Bellwood 1981 [Leang Buidane, Talaud]). It also matches Solheim's Kalanay (Metal Age) pottery style from the Philippines (Solheim 1964). This is a very widespread horizon in the Halmahera region and seems to terminate the local pre-ceramic in these islands; there may be a strong element of agricultural dispersal as well as trade factors involved in its spread. It also replaces the red-slipped ceramic horizon on Kayoa and is there associated with glass beads and fragments of bronze and iron, together with jar burials of a very widespread eastern Indonesian and Philippine Metal Age type. It seems hardly coincidental that the Northern Moluccas also entered the Spice Trade about 2,000 years ago according to Classical and Indian records (a date which might be modified by claims of Middle Bronze Age cloves from Syria, should they be verified: see Buccellati and Buccellati 1983; see also Mahdi, Ch. 17, this volume). This entry was perhaps initiated by western Indonesian traders linked to southern Indian buyers from ports such as Arikamedu in Tamil Nadu (Ardika and Bellwood 1991).

If the above observations are broken down into essentials, then three phases seem fairly clear. The first is the culturally rather isolated pre-ceramic phase of the Pleistocene and Early to Mid-Holocene, the second is the phase of the Philippine- and eastern Indonesia-related early red-slipped pottery of the second and early first millennia BC represented at Uattamdi, and the third is the phase of the widespread 'Indonesian Metal Age' style pottery dating everywhere in the Northern Moluccas from about 2,500 years ago. Can we see such patterns of regional isolation versus regional interaction in the linguistic records?

WEST PAPUAN AND AUSTRONESIAN LANGUAGE SUB-GROUPS IN THE NORTHERN MOLUCCAS

The languages of the Halmahera region belong to two genetically unrelated phyla – West Papuan in the north (one of several Papuan language families in the New Guinea region), and Austronesian (a language phylum of ultimate mainland Asian origin via Taiwan) in the south (Figure 5.1). The only linguist to offer an overall classification and discussion of the West Papuan languages has been Voorhoeve (1988). His research has been based on lexicostatistics rather than full use of the comparative method to plot shared innovations, but the results still offer a picture of considerable interest.

The Papuan languages of Northern Halmahera are classified into a North Halmahera Stock by Voorhoeve. This stock belongs to the West Papuan Phylum, which evidently has its greatest level of diversity in the Bird's Head region of West New Guinea. It is possible, but presently cannot be proved, that the West Papuan languages of Halmahera were taken to the island by migrants from New Guinea. How long ago might this have occurred? Since glottochronology in this region is unlikely to be a reliable technique owing to gross differences in rates of language change (Blust 1993: 245), all I can offer is a guesstimate of several millennia based on a minimum shared cognate percentage between all pairs of languages in the North Halmahera Stock of only 21 per cent (a figure perhaps inflated by hidden borrowing: Voorhoeve 1988: 182). It is quite possible that the West Papuan languages of Halmahera were first introduced there well back in the pre-ceramic period, indeed perhaps as long as 30,000 years ago, although there is no visible patterning in the phylum which could possibly support or refute such an enormous antiquity. However, the fact that the Papuan phyla in New Guinea clearly predate the arrival of Austronesian languages in that region makes it very likely that the same situation holds for the Northern Moluccas.

Remarkably, among the North Halmahera Stock languages, the largest sub-family (the Halmahera Sub-family) contains very closely related languages and covers the whole of Northern Halmahera, Ternate and Tidore, except part of the island of Makian (Wurm and Hattori 1983, Map 45: Voorhoeve calls it the North Halmahera Family). Within it all languages share over 65 per cent of basic vocabulary (100-word list). The second sub-family contains only the West Makian isolated language which shares 21–28 per cent of cognates with the languages in the Halmahera Sub-family. This suggests a situation in which the West Makian language has remained in place for many millennia, but earlier Papuan languages in Northern Halmahera and Morotai have been completely replaced by a recent language radiation or levelling (an interpretation also followed by Yoshida 1980: Fig. 1). This radiation presumably took place some time within the past 2,000 years. It could have been stimulated in part by a dispersal of agriculturalists into remote forested terrain previously inhabited by foragers, a dispersal perhaps in turn stimulated by the demands of the spice trade. The very wide occurrence across Indonesia after 2,000 years ago of characteristic incised pottery of Indonesian Metal Age style also surely reflects increased communication and population movement from this time onwards.

Another very interesting observation made by Voorhoeve (1988: 194) is that the North Halmahera Stock languages of Ternate, Tidore, West Makian and Sahu, all located in the mid-western geographical portion of the Halmahera group, have adopted many elements of Austronesian grammar. Some of this borrowing is also claimed by Voorhoeve to be quite ancient, although no exact date is suggested. The locations of these four languages, perhaps not coincidentally, are all quite close to the island of Kayoa with its so-far unique assemblage of early Austronesian affinity dating from 3500 BP

in the Uattamdi shelter. Was the initial enclave of Austronesian speakers at about 3500 BP localized to a relatively small area focused on the chain of small islands from Bacan up to Ternate?

There is no simple answer to this question from an Austronesian linguistic viewpoint at present since much basic research is still needed. According to Blust (1978, 1993), the Austronesian languages of the Halmahera region, which presumably at some time replaced pre-existing Papuan languages, belong to the South Halmahera sub-group of the higher-order South Halmahera-West New Guinea (SHWNG) sub-group of Austronesian.[2] The SHWNG sub-group is defined by about thirteen linguistic innovations (Blust 1978, 1983–4, 1993), sufficient perhaps to suggest a period of linguistic unity prior to expansion and diversification. Whether this phase of unity occurred in the Moluccas or Irian Jaya is at present unclear. The sub-group contains, according to Blust, two lesser-order sub-groups; one (Southern) comprising the languages of eastern Makian, Kayoa and the southern mainland of Halmahera (Giman); the other (Central-Eastern) comprising the languages of Eastern Halmahera (including Weda). The archaeological enclave of early Austronesian-related settlement located on Kayoa lies within the area of the Southern sub-group, which one might thus expect from archaeological data to present more internal diversity, hence greater time-depth, than the Central-Eastern sub-group. Unfortunately, the only available lexicostatistical percentages for the Halmahera region are those given by Grimes and Grimes (n.d.). These do not extend to any regions of related languages in Irian Jaya so it is not possible to state which of the Halmahera sub-groups represents the earliest area of Austronesian settlement. The languages in the two sub-groups (Southern and Central-Eastern) share as few as 33 per cent of cognates on a 195-word list, but all this tells us is that Austronesian linguistic differentiation within the Halmahera islands has been continuing for several millennia.

Despite these problems of proceeding further with Austronesian linguistic prehistory in the Northern Moluccas, there still remains the important lexicostatistical observation of Grimes and Grimes (n.d.) that the languages of the eastern arm of Halmahera (Maba, Buli, Patani) are very closely related. They probably represent a relatively recent spread within the past 2,000 years, as suggested already for the North Halmahera Sub-family of the Papuan languages further north. Like these, the eastern Halmahera Austronesian languages might have spread through a combination of causes, including population movement, trade, and perhaps even some degree of forcible population replacement in recent centuries (Andaya 1993).

Some conclusions about linguistic–archaeological correlations for the Northern Moluccas are presented in Table 5.1. It is perhaps worth adding that the only available genetic analysis for this region (described in Bhatia *et al.* 1995) has examined the Ternate and Galela populations, who speak Papuan languages in the Halmahera Sub-family. Given the linguistic affinities of these languages with the West Papuan Phylum spoken in New Guinea it might

be expected that these populations would group with New Guineans in terms of their genotype, but in fact they do not. They group instead with the Indonesian populations to the west, whom they also resemble most closely phenotypically. Clearly, this is a situation where languages have been maintained throughout a long period of fairly intense intermarriage between Austronesian groups of Asian origin and Papuan groups of western Pacific origin, intermarriage which has shifted the biology of the Papuan speakers closer to an Asian form. Much of this intermarriage can be expected to have occurred during the past 500 years of intense trade, tribute collection and political interference mediated through the sultanates of Ternate and Tidore. Even though the native languages of these two sultanates were Papuan, their cultural heritage was very much derived from western Indonesian sources, a point stressed by the social anthropologist Jos Platenkamp (1990). However, some degree of Austronesian–Papuan intermarriage can in theory also have occurred much earlier, from 3,500 years ago.

DISCUSSION

This chapter represents preliminary ideas which may be modified as more research data come to hand and the conclusions are therefore of a general and theoretical nature. First, if the archaeological and linguistic data for this small region are considered independently, there are some fairly obvious correlations which do not involve circularity of reasoning. Understanding prehistory in the Moluccas, or anywhere else in the Holocene world for that matter, is not just a task for archaeologists (although for more ancient time-spans the archaeologists, of course, tend to hold the field more securely).

Second, detailed examination of the relationships between populations through time in the Northern Moluccas, using both archaeological and linguistic data, suggests just how 'porous' these groups were when opportunities for interaction were made available. But, as stressed at the beginning of this chapter, many processes which encourage such local porosity become reduced to a very minor and localized scale when compared with human patterning on a continental scale. Here, some archaeologists have been remiss in ignoring the significance of rapid population expansion, fuelled by rates of population growth which in colonizing situations were probably extremely high. For example, microcosmic prehistory in the Northern Moluccas tells us that cultural and linguistic interactions and perhaps even language shifts must have been of considerable importance there during the past 2,000 years of spice trade and the growing domination of the Halmahera mainland by the islands of Ternate and Tidore and their neighbours. But macrocosmic prehistory on the scale of the whole of Austronesian tells us that language shift cannot have been an important process of language spread *overall*. Neither, I suspect, has adoption by hunter-gatherers been a very important process in the overall spread of agriculture, even though the ancestral Papuan populations

of northern Halmahera might well have adopted many crops and elements of field agriculture from incoming Austronesian populations. In interpreting human prehistory, scale, patterns and long-term plus world-wide perspectives all matter, even on the most microcosmic scale.

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NOTES

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2 Language grouping terminology differs somewhat between the Papuan and Austronesian languages, reflecting to some extent the usage of lexicostatistics versus shared innovation methodology for analysis. The term 'family' in the Austronesian literature corresponds roughly to the term 'phylum' for Papuan.

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6 *Sequencing and dating linguistic events in Oceania: the linguistics/archaeology interface*

MALCOLM ROSS

ACRONYMS

CP	Central Papuan	PPT	Proto-Papuan Tip
NWCP	Nuclear West Central Papuan	PT	Papuan Tip
PCP	Proto-Central Papuan	RKM	Roro/Kuni/Mekeo
POc	Proto-Oceanic		

INTRODUCTION

The starting-point for this chapter is the belief that, even though linguists have intuitions about how fast various languages have changed, these intuitions are decidedly unreliable, at least when applied to individual languages and small groups of languages. The only linguistic tool which linguists have as a test of their intuitions is glottochronology, and this has proven far from reliable. What the linguist *can* do, however, using the linguistic comparative method, is to place linguistic events in a sequence, relying on the archaeologist to provide absolute dates for archaeological events with which these linguistic events can be correlated.

Before turning to sequencing and correlation, it is appropriate to say briefly why I find glottochronology inadequately reliable. Glottochronology was the brainchild of Morris Swadesh, and important early writings on the subject include Swadesh (1950, 1952, 1955), Lees (1953) and Gudschinsky (1956). Early criticism came from Hoijer (1956). Hymes (1960) provides a well-known summary of work until that date (a number of Swadesh's papers were republished as Swadesh 1971). Glottochronology is dependent on the technique known as 'lexicostatistics'. The lexicostatistician uses the percentages of apparent cognates (that is, items in related languages which appear to be directly inherited from a common ancestor) shared by pairs of languages on a standard word list to arrive at a 'family tree' of those languages. Glottochronology is a technique for dating the nodes in that family tree. Both

techniques, however, depend on the assumption that words with the meanings on the standard word list are replaced at a constant rate. As a number of scholars have shown (e.g. Bergsland and Vogt 1962 for North Germanic, South Caucasian and Eskimo; Blust 1981 for Austronesian), this assumption is far from valid. The words on a standard word list evidently vary in their rate of replacement from language to language according to the conventions which govern language use (Bergsland and Vogt refer to word taboo) and according to the social history of a language's speakers, especially the amount and kind of contact which they have had with speakers of other languages (such that contact results in bilingualism and lexical borrowing). There *are* cases, like the languages of Polynesia, where lexicostatistics – and therefore glottochronology – seem to work quite well (Pawley 1994), but these are cases where there has apparently been almost no contact with languages outside the family, and such cases are really quite rare. A good account of lexicostatistical and glottochronological approaches is given by Embleton (1991). Bynon (1977: 266–72) provides a brief critique of lexicostatistics from the standpoint of the comparative method.

No doubt generalizations about the rates of certain kinds of changes can be made with regard to large populations of languages, but within such a population rates of change are distributed along a curve approximating (I assume) a normal distribution. The problem with attributing a certain rate of change to a single language or small group of languages is that we have no idea where it occurs on the curve.

My concern here, however, is to show how the linguistic comparative method works to reconstruct a sequence of linguistic events which can be correlated with a reconstructed archaeological event sequence. This presupposes, of course, that event sequences of both types have been reconstructed, and that is all too rare in western Melanesia, the area in which I work. In Volume I, I enumerated the various *kinds* of events that a linguist can reconstruct; here the focus is on the *sequence* of reconstructed events.

Rather than discuss the reconstruction of linguistic events in the abstract, it will be easier to talk about them in relation to the main example to be presented in this chapter, the Oceanic Austronesian languages of Central Papua, and it is to an introduction to these languages that I now turn.

THE AUSTRONESIAN LANGUAGES OF CENTRAL PAPUA

The languages which are the central concern of this chapter belong to the Oceanic sub-group of the vast Austronesian phylum. The phylum includes the aboriginal languages of Taiwan, the languages of the Philippines, most languages of Malaysia and Indonesia except in the Indonesian province of Irian Jaya, all of Madagascar, many languages of the coasts and islands of Irian Jaya and Papua New Guinea, and the languages of the Pacific islands. The Oceanic sub-group includes almost all Austronesian languages spoken in

Melanesia (i.e. Irian Jaya, Papua New Guinea, the Solomon Islands, Vanuatu, New Caledonia and Fiji), Polynesia and Micronesia. Exceptions to this generalization are a sprinkling of non-Oceanic Austronesian languages in Irian Jaya and western Micronesia. The geographic continuity of Austronesian languages is massively interrupted by the immense and diverse collection of 'Papuan' or non-Austronesian languages spanning an area from eastern Indonesia through Papua New Guinea into the Solomons. Since little is known about the genetic relationships of these non-Austronesian languages to one another, and it is virtually certain that they do not constitute a single phylum, I refer to them here simply as 'non-Austronesian' (a survey is provided by Foley 1986). Throughout this area, however, are scattered a number of Oceanic languages whose speakers are often in contact with speakers of non-Austronesian languages.

The internal relationships of the Austronesian phylum as a whole and of much of the Oceanic sub-group need not concern us here (for summaries, see Pawley and Ross 1993, 1995; Ross 1994c). The Oceanic languages of Central Papua (CP languages) form a sub-group within the Papuan Tip (PT) cluster, itself a second- or third-order sub-group within the Oceanic group (Ross 1988: Ch. 6 describes this sub-grouping in some detail; Ross 1989 is a non-technical summary).

The CP and PT language groups are shown in Figure 6.1. I have chosen deliberately to use the abbreviations 'CP' and 'PT' in the text, in order to avoid the repeated use of the word 'Papuan', whose better known and quite different application is to the non-Austronesian languages I have just mentioned.

Speakers of the majority of Oceanic-speaking groups in western Melanesia, including those which concern us here, live in villages, usually with only a few hundred inhabitants, sometimes consisting of a collection of tiny hamlets of only two or three families each, related by ties of descent, marriage and landholding. The traditional maximum political unit was the village, governed by the oldest generation of males. This means that when pressures of relationship or population or whatever caused a group of people to leave their village and establish a new one, they typically created a new political and social unit and with it a new identity for themselves. Since language is emblematic of identity (Grace 1975), the creation of a new identity brought with it a pressure over time to create a 'new' language, that is, to emphasize and perhaps exaggerate linguistic differences from one's neighbours. In some cases, at least, these differences had arisen as a result of moving to a new location and thereby coming into contact with (and often becoming bilingual in) another, often non-Austronesian, language (Ross 1996).

Typically, Oceanic-speaking villages in western Melanesia are located on the coast, and have traditionally practised a mixture of subsistence agriculture, fishing, and some hunting and gathering, the latter both in the bush and on the reef. Before contact with Europeans there were no metal artefacts except on the westernmost fringe of Melanesia. Communities were sometimes partially specialized in a technical skill (e.g. pottery) or a subsistence technique

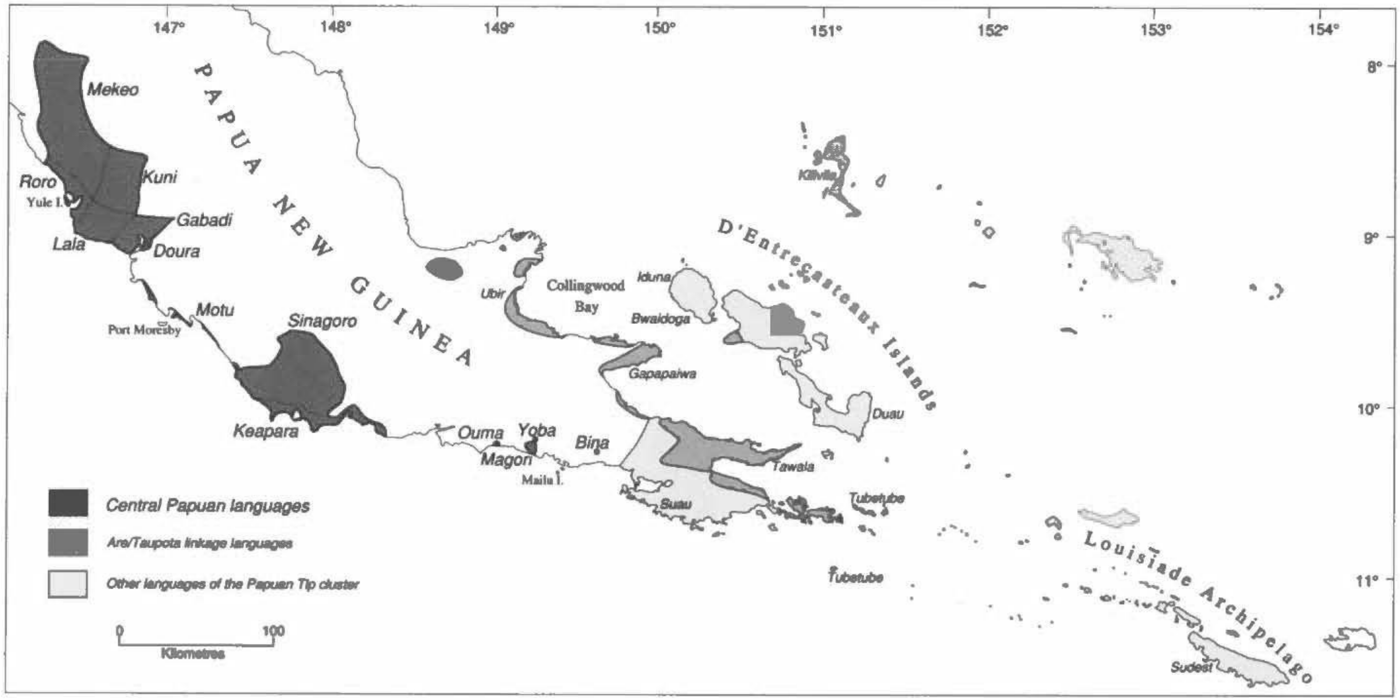

Figure 6.1 Papuan Tip cluster of Oceanic Austronesian languages

(e.g. harvesting and processing sago) and traded their products with other communities, sometimes over quite long distances. The CP Motu, for example, regularly travelled on trading voyages to the Gulf of Papua (to the northwest of the Roro; see Figure 6.1). As the map shows, a number of CP-speaking communities have moved inland and therefore no longer engage in marine activities.

RECONSTRUCTING LINGUISTIC AND COMMUNITY EVENTS

The term 'linguistic event' as used here refers to a change in the language itself. This may be a sound change (a change in the way the language is pronounced), a lexical change (for example, the borrowing of a word from another language), or any one of a number of different kinds of systemic changes. Linguistic events quite often reflect changes in the life of the speech community, changes which are therefore inferrable from linguistic events. I refer to these changes as 'speech community events', or 'community events' for short. It is these changes which can be related to archaeological events.

Using the comparative method, linguists can reconstruct community events of two broad kinds. The first consists of those events which are traditionally modelled in a 'family tree' diagram, the second of events entailing contact between distinct languages or dialects.

Language fissure

A family tree diagram shows language fissures, that is, occasions when one community has split into two or more communities so that the inherited versions of their shared language have progressively diverged, becoming different languages. Thus at some time in the past, the ancestors of the speakers of Proto-CP (PCP, the language ancestral to present-day CP languages) separated themselves geographically and socially from other speakers of some PT language somewhere around the southeast tip of Papua, and moved westwards along the south coast of Papua. They formed a new community, and over time their language became increasingly different from the language of those who stayed behind.

But we need to take care that the family tree metaphor, derived especially from Schleicher's (1863) Darwinian view that languages are natural organisms (for discussion see Ross and Durie 1996), does not cause us to lose touch with the fact that languages have speakers and speakers live in communities. Language diversification does not always entail sharp geographic and social separation. In the western Melanesian context, the process has often been more gradual, with new communities of speakers remaining in quite frequent contact with the old and retaining kinship and other links with them, so that the speech traditions of the old and new communities have diverged from each other only slowly; that is, a single language first diversifies into a linkage

of lects, such that the emblematic linguistic features of each community are limited to a few well-recognized habits of pronunciation and pieces of vocabulary. (I use the term 'lect' and the corresponding adjective 'lectal' to cover both 'language' and 'dialect', since there is no objective way to draw a boundary between the referents of the two terms. I use 'linkage' to cover what others might call a lectal 'chain' or 'network', since the latter term is usefully reserved for 'social network', discussed in Ross, Ch. 13, Volume I). The traditional family tree diagram does not allow us to distinguish between separation and lectal diversification.)

Figure 6.2 uses the convention (introduced in Ross 1988: 9–11 and *passim*) of a broken horizontal double line to indicate a lectal linkage, and thereby introduces somewhat more flexibility into the tree-diagramming of language relationships. Thus it shows that the PT languages at one time formed a large linkage, which became separated into four parts. Two of these parts, Proto-Suauic and PCP, probably resulted from the separation of a community from the rest of the linkage, while the other two, the North Mainland/D'Entrecasteaux and the Kilivila/Louisiades linkages, probably resulted from a gradual reduction in social interaction between parts of the remaining PT linkage. Similarly, the diagram indicates that the speakers of PCP appear to have split into three separate communities, whose speech traditions diverged to become, from east to west, Proto-Ouma/Magori, Proto-Sinagoro/Keapara, and Proto-West CP (an alternative interpretation will be offered on p. 152). Proto-Sinagoro/Keapara gradually diverged into a linkage (the Sinagoro/Keapara linkage), then this linkage slowly divided into the largely marine-orientated Keapara linkage and the savannah-orientated Sinagoro linkage.

The fate of Proto-West CP appears from Figure 6.2 to be considerably more complicated, but this is largely an artefact of my attempt to show the details of what happened through a diagrammatic medium which is lacking in appropriate subtlety. What I have sought to show is that Proto-West CP first diversified into a lectal linkage and then that, as this linkage increased (I infer) in geographic extent, one community after another became sufficiently separated in social terms from the linkage for its speech tradition to develop into a distinct language. This happened first to Motu, then to Gabadi, then to Doura and Lala, and finally to Roro, Kuni and Mekeo.

We turn now to the question of how linguists using the comparative method employ it to reconstruct past changes in language systems, i.e. linguistic events, and use them to reconstruct community events. The basic technique is to reconstruct the proto-language for a group of related languages (i.e. the common ancestor of the group), then to identify the innovations which characterize sub-groups within that group.

For example, it is quite easy to show that all the Oceanic languages in the CP group share a set of innovations relative to Proto-PT (henceforth PPT), the language ancestral to all PT languages. The probability that all the CP languages would have undergone this set of innovations independently of

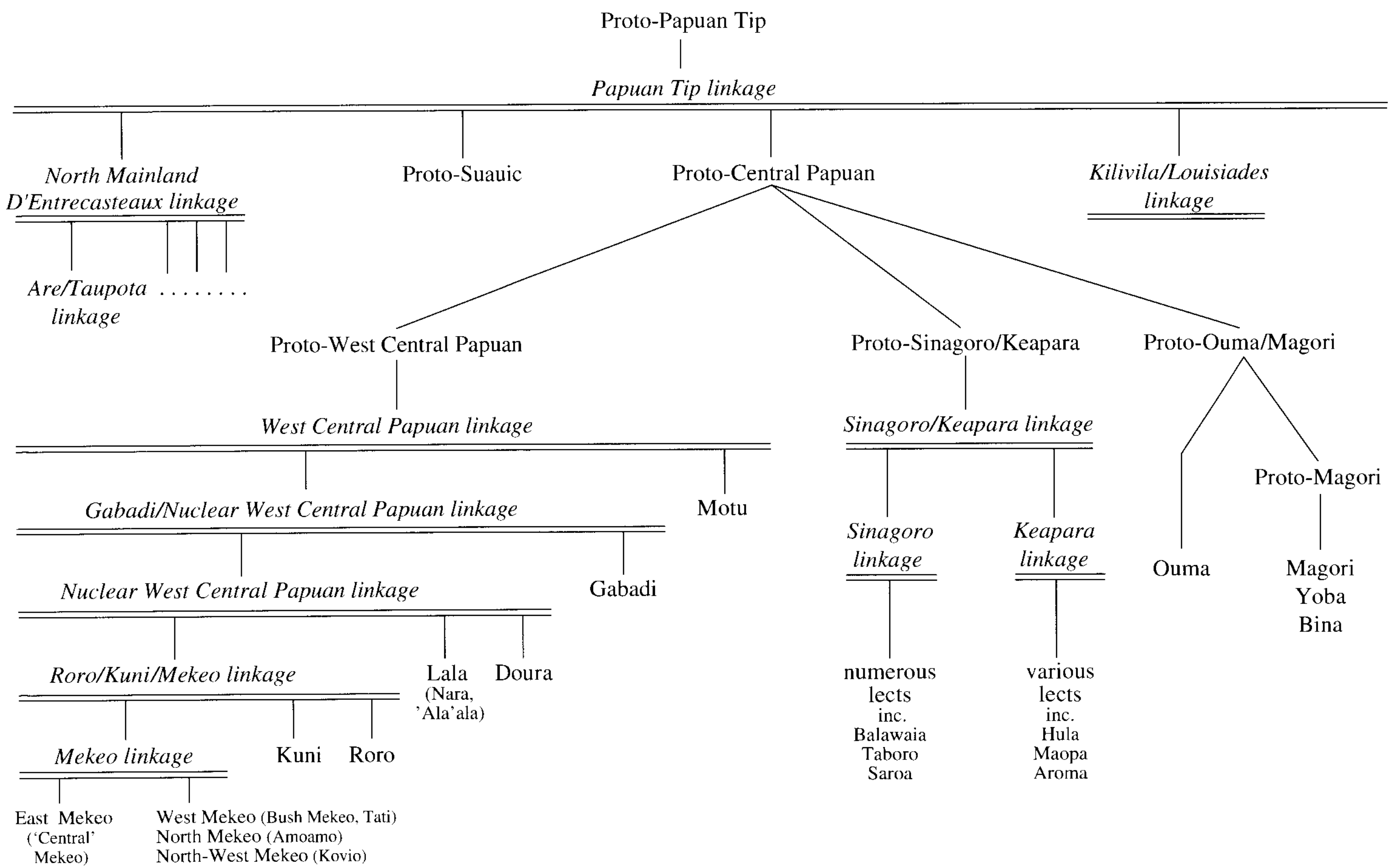

Figure 6.2 Austronesian languages of Central Papua: approximate genetic tree

each other is for practical purposes zero. The only explanation for this sharing of innovations is that they occurred in a single language, PCP, and that all the languages which reflect them are descended from that language, thereby forming a sub-group.

The innovations themselves are, of course, linguistic events, but by inferring the existence of PCP we presuppose two community events: the separation of the ancestors of the speakers of PCP from the speakers of other PT languages, and the break-up of PCP into the languages of the CP group. The innovations occurred between these two events.[1]

The reconstruction of these community events presupposes the accurate reconstruction first of PPT, and second, of the innovations which resulted in PCP. It therefore behoves us to examine how such a reconstruction is made and why we may consider it reasonably reliable.

The tenet upon which the comparative method largely rests is that the sounds of a language change in a regular and exceptionless manner.[2] It is less widely recognized that it also rests on the assumption that we can infer the direction of most sound changes.

The linguist's first step in reconstructing the sound system and the vocabulary of a proto-language is to collect sets of words which are apparently related to each other across languages. Table 6.1 shows a few such sets drawn from a selection of PT languages. There are gaps in these sets where appropriate cognates have not been found in the database, perhaps because a cognate does not occur in the relevant language, perhaps because it simply has not

Table 6.1 Cognate sets illustrating the reconstruction of Proto-PT *b

	'butterfly'	*'coconut husk'*	*'hornbill'*	*'crane'*	*'flying fox'*	*'sago'*	*'bake'*
PPT	*bebeq	*bunu	*binam	*boqe	*mariboŋi	*rabia	*gabu
Dobu	...	bu-bula [a]	binama	...	...	labia	gabu
Bwaidoga	bebewa	bu-bu [a]	...		...	labia	...
Iduna	bebewa	bu-bu [a]	...	...	...	...	gabu
Gapapaiwa	...	...	...		...	rapia	kapu-ni
Tubetube	bebe	...		...	maliboi	labia	gabu
Duau	...	bunu	...		malibon	...	gabu
Kilivila	beba	...	...	boi	...	yabia	gabu
Sudest	bebi	...	...	boi	...	yabia	...
PCP	*bebe	*bunu	*bina	*boɣe	*malibo(ŋ)i	*labia	*gabu
Balawaia	kaubebe	bunu	bina	boɣe	maliboi	labia	ɣabu
Hula	pepe	punu	pina	poɣe	...	lapia	kapu
Motu	kaubebe	bunu	bina	boe	mariboi	rabia	...
Lala	ebebelo	punu	...	boe	maliboʔi	...	ŋabu
Roro	peropero	punu	...	poe	...	...	...
E. Mekeo	fefe	funu	...	foe	...	...	...

Note: [a]The Dobu, Bwaidoga and Iduna forms reflect a form *bu-bun, i.e. with reduplication of the initial syllable and loss of final *-u.

been collected. In the compilation of putative cognate sets, account also has to be taken of changes in meaning. In the 'crane' column, for example, the Balawaia, Lala and E. Mekeo words all mean 'heron', Lala 'white heron', and Hula 'white ibis', but these are readily explicable.

The next step is to work out sound correspondences from the putative cognate sets. Table 6.1 consists of cognate sets in which a *b* in most languages in the table corresponds to a *p* in Gapapaiwa, Hula and Roro and an *f* in E. Mekeo. The regularity of the correspondence across a number of cognate sets (only Lala *punu* is 'irregular' here) reflects the regularity of sound change. In this case there is little doubt that the PPT phoneme was **b* (an asterisk indicates a reconstruction) and that this **b* changed to become Gapapaiwa, Hula, Roro *p*, Pre-Mekeo **p*, the latter later becoming E. Mekeo *f*.

This process is repeated for each sound correspondence until the sound system of the proto-language has been tentatively reconstructed (the resulting sound correspondences for the CP languages are tabulated in the Appendix). The reconstructed sound correspondences and sound system can then be applied to the reconstruction of the words of the proto-language, in this case PPT **bebeq* 'butterfly', **bunu* 'coconut', **binam* 'hornbill' and so on. Since the PT group is part of the much larger Oceanic group, Oceanic languages outside the PT group also provide external evidence for the reconstruction of PPT words which were inherited from Proto-Oceanic.

The phonological innovations relative to PPT which are shared by all CP languages and are accordingly attributed to PCP are as follows (POc = Proto-Oceanic):[3]

1 (i) POc/PPT **u* becomes PCP **i* after POc/PPT *-*ol*-, *-*ul*-, *-*al*-;
(ii) POc/PPT **l* is lost before POc/PPT **i* and **u* [this environment is fed by (i)];
(iii) POc/PPT **l* and **y* merge as PCP **y* before POc/PPT **a*, **o*.
2 POc/PPT **s* becomes a stop or a flap in CP languages (PCP **r*).
3 POc/PPT word-final consonants are lost in absolute final position.

Of these, the most significant is (1), since it entails a sequence of changes affecting POc/PPT **l* which has occurred nowhere else in Oceania. Innovation 1(i) is illustrated in Table 6.2. Innovation 1(ii) is illustrated both by the examples in Table 6.2 and those in Table 6.3 (Innovation 1(ii) is fed by 1(i)). Table 6.4 contains examples of Innovation 1(iii). Doura *mara* (for expected ***mala*) and Maopa *vuara* (for expected **vuaða*) are unexpected and unexplained forms. Each of Tables 6.2, 6.3, and 6.4 also give reflexes in a few PT languages outside the CP group in order to show that these languages are not affected by CP innovations.

The sound changes presented as Innovations 1, 2 and 3 are linguistic events, but the very fact that all the CP languages reflect these changes (as well as some grammatical changes) is enough for us to infer that all of them are descended from an exclusively shared parent language, PCP. That is, they are sufficient for us to infer a community event, namely that the forefathers

Table 6.2 Cognate sets illustrating Innovation (1i)
POc/PPT **u* becomes PCP **i* after POc/PPT *-*ol*-, *-*ul*-, *-*al*-

	'three'	*'egg'*	*'hair'*	*'unit of ten'*	*'Spanish mackerel'*
PPT	*tolu	*qatolur	*pulu	*ŋa-pulu	*walu[a]
Ubir	ton	. . .	bu-buni	. . .	. . .
Tubetube	tolu	. . .	. . .	na-ulu	. . .
Suau	. . .	katino	ulu-	. . .	. . .
PCP	*toi	*ɣatoi	*vui	*ŋa-vui	*vai
Magori	. . .	atoʔi	. . .	. . .	. . .
Balawaia	toi-toi	ɣatoi	ɣui	. . .	. . .
Maopa	oi-oi	aoi	vui	. . .	. . .
Hula	koi-koi	aoi	vui	. . .	. . .
Motu	toi	ɣatoi	hui	a-hui	vai(na)-vai(na)
Gabadi	koi	. . .	bi	a-vui	. . .
Doura	(au)kui	akui	hui	a-hui	. . .
Lala	koi	akoi	viu	ŋa-vui	. . .
Roro	. . .	ahoi	bui	. . .	. . .
Kuni	koi	awoi	bui	. . .	. . .
E. Mekeo	oi(do)	aʔoi(na)	pui	. . .	. . .

Note: [a]The reconstruction of POc/PPT *walu is attested by Oceanic reflexes outside the Papuan Tip region (Ross 1994b).

Table 6.3 Cognate sets illustrating Innovation (1ii)
POc/PPT **l* is lost before POc/PPT **i* and **u*

	'five'	*'ear'*	*'barter'*	*'centipede'*	*'calm'*	*'banana or taro sucker'*
PPT	*lima	*taliŋa	*voli	*qalivan	*malino	*juli(q)
Dobu	nima	tena	. . .	ganihana	. . .	suli 'taro'
Tawala	. . .	taniga	. . .	. . .	. . .	huni 'taro'
Tubetube	. . .	tena	pali	kalia	. . .	. . .
PCP	*ima	*taiŋa	*voi	*ɣaiva	*maino	*dui 'banana shoot, plant'
Ouma	ima	taʔa	. . .	gaiva	. . .	. . .
Magori	ima	. . .	. . .	aiva	. . .	. . .
Yoba	. . .	taia	. . .	. . .	. . .	. . .
Bina	. . .	taia	. . .	. . .	. . .	. . .
Balawaia	(ɣ)ima	teɣa	voi-voi	ɣaiva	maino	. . .
Maopa	ima-ima	eɣa	voi-voi	ɣaiva	maino	. . .
Hula	ima-ima	kea	voi(a)	aiva	. . .	rui- 'young banana shoot'
Motu	ima	taia	hoi	aiha	. . .	dui 'banana plant'
Gabadi	ima	kai	. . .	. . .	maino	. . .
Doura	ima	kaia	. . .	. . .	. . .	. . .
Lala	ima	kaia	. . .	. . .	maino	. . .
Roro	ima	haia	. . .	. . .	. . .	tsui(ara) 'k banana'
Kuni	ima	kaia	. . .	aiba	. . .	. . .
E. Mekeo	ima	aina	. . .	aipa	. . .	ui 'domestic banana plant'

Table 6.4 Cognate sets illustrating Innovation (1iii)
POc/PPT **l* and **y* merge as PCP **y* before POc/PPT **a*, **o*

PPT	*laqo	*laŋo	*yaŋin	*yaŋo	*(s,j)alan	*maya	*vuqaya	*vitolo
Maisin	. . .	. . .	. . .	. . .	. . .	mee	. . .	vitoro
K	'go'	'housefly'	'wind'	'yellow'	'road'	'tongue'	'crocodile'	'hungry'
alokalo	naɣo	. . .	yagina	yaɣo-yaɣo-	. . .	. . .	. . .	. . .
Dobu	nao	. . .	yagila	. . .	. . .	mea-	uaya	. . .
Wedau	. . .	. . .	ðaɣina	. . .	. . .	. . .	iwaðo	. . .
PCP	*yaɣo	*yaŋo	*yaŋi	*yaŋo-	*rala	*maya	*vuɣaya	*vitoyo
Ouma	. . .	. . .	. . .	. . .	raea	. . .	uaya	. . .
Magori	eao	. . .	. . .	. . .	rae	. . .	uae	. . .
Yoba	yau	. . .	. . .	. . .	. . .	. . .	. . .	. . .
Bina	yao	. . .	. . .	. . .	. . .	. . .	. . .	. . .
Balawaia	aɣo	. . .	aɣi	. . .	. . .	mae	ɣuaa	vito
Maopa	ðao	. . .	ðaɣi	. . .	raða	maða	vuara	. . .
Hula	. . .	. . .	. . .	. . .	. . .	mae	vuɣaa	vio
Motu	lao	lao	lai	lao(ba-)	dala	mala	huala	hitolo
Gabadi	. . .	ao(kama)	ai	ao(bauba)	gere(ʔa)	mara	uaa	. . .
Doura	. . .	lao(kama)	. . .	. . .	. . .	mara	. . .	. . .
Lala	. . .	lalo(maka)	lani	lao(ba-)	dala	mala	vuala	vikolo
Roro	ao	ao(maha)	rani	. . .	taea(ra)	maea	buaea	. . .
Kuni	. . .	a(m)o(aka)	. . .	yao(fa-)	daya	maya	buaya	. . .
E. Mekeo	lao	ano(ma)	. . .	lao(fa-)	kea(na)	mala	ufala	. . .

of the speakers of PCP separated themselves from other speakers of other PT languages and remained a single speech community until after these linguistic changes had taken place.

Using linguistic events to infer the one-time existence of a shared parent language is a relatively simple matter. In this case the fact that the CP languages exclusively share Innovations 1, 2 and 3 leads to the inference of PCP. But to infer the one-time existence of a lectal linkage from linguistic events can be a rather more complex task. A lectal linkage consists of a collection of speech communities with speech traditions each of which is sufficiently similar to its neighbours to be mutually comprehensible with them, but which are none the less sufficiently different from each other for their speakers to recognize the differences (and to treat some of them as emblematic). When an innovation occurs in community A, it may well spread to its neighbours, say B and C. Some other innovation may arise, say, in community D and spread from there to C and E. The result is an overlapping pattern of innovations (on this subject, see Ross 1996).[4]

It lies beyond the scope of this chapter to give a detailed example of this kind of pattern, but the clearest example is the Sinagoro/Keapara linkage. In Figure 6.2 Proto-CP is shown breaking into three discrete speech communities (Proto-Ouma/Magori, Proto-Sinagoro/Keapara and Proto-West CP), but the alternative interpretation in Figure 6.3 is probably more accurate.

Figure 6.3 says that PCP broke up into a linkage of lects (the CP linkage), and that Proto-Gabadi/Nuclear West CP, (Pre-)Motu, and Proto-Ouma/

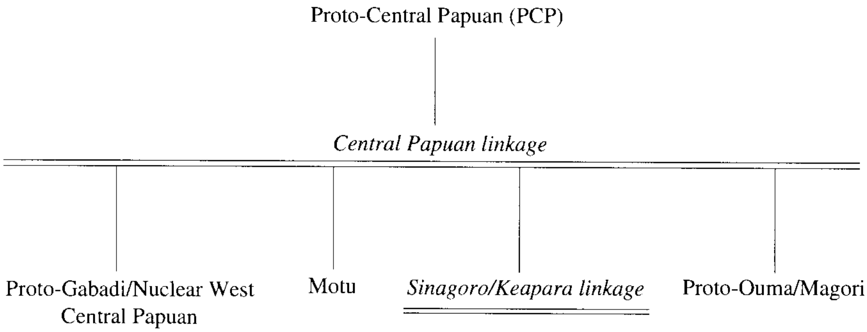

Figure 6.3 Alternative view of the early sub-grouping of Central Papuan languages

Magori each broke away from this linkage to become separate languages, while the lects in part of the linkage remained in contact with each other as the Sinagoro/Keapara linkage, diverging only very slowly to form present-day Sinagoro and Keapara linkages (Dutton 1970).

Figure 6.3 differs from Figure 6.2 in its assertions first that there never was a unitary language which we can label 'Proto-Sinagoro/Keapara', and second, that Motu is a lone offshoot from the CP linkage, not a part of the West CP linkage (in this respect it echoes the conclusion of Pawley 1975). These new assertions are related to each other. There is little doubt about the unity of the Sinagoro/Keapara linkage as a linkage of closely related lects (Dutton 1970; Lynch 1983). However, only one sound change is common to the whole linkage. This is the merger of PCP *γ and *η as Sinagoro/Keapara *γ. But this merger is also reflected in Motu. Motu, however, shares certain innovations of a grammatical nature with the remaining West CP languages (Ross 1994b). We thus have the classic pattern of overlapping innovations from which we may infer that there was once a CP lectal linkage.

Since we are left with no innovations common to all Sinagoro and Keapara lects, despite their mutual similarity, we can no longer infer that there was a discrete Proto-Sinagoro/Keapara. However, there are plenty of overlapping innovations among the lects of the Sinagoro/Keapara linkage which show that it has long been a linkage, perhaps since the diversification of PCP itself.

Whether Proto-Ouma/Magori was once a part of this network, as Figure 6.3 indicates, or broke off before the formation of the CP network, remains an unanswered question, largely because these moribund languages, heavily affected by their contact with non-Austronesian neighbours, do not offer enough reflexes of PCP forms for us to make such delicate inferences.

As I have hinted here, sub-grouping method is applied recursively to form a 'family tree'. The erstwhile existence of PPT can be inferred from the fact that all PT languages reflect certain innovations relative to Proto-Oceanic (Ross 1988: Ch. 6; 1992), of PCP from the fact that all CP languages reflect the innovations listed earlier relative to PPT, and, as Figure 6.2 shows, the method can be applied several times over to reconstruct the history of

the West CP languages. In this way, a sequence of linguistic events can be reconstructed.

Before we turn to the matter of sequencing, however, it is appropriate to note that the comparative method does not necessarily generate the same tree as the application of lexicostatistics. Thus the lexicostatistical computations listed by Pawley (1975) show that Doura shares its highest percentage of shared cognates with Motu, implying that Motu is Doura's closest relative, but the application of the comparative method summarized in Figure 6.2 shows that Doura's closest relatives are the other members of the Nuclear West CP linkage, of which Motu is not a member. Since the comparative method uses the data to reconstruct a sequence of past linguistic events and to infer community events from them, it can be regarded as fundamentally more reliable than lexicostatistics, which produces a table of percentages which allow no detailed inferences about *linguistic* events and which may be vitiated by variable rates of lexical replacement and by borrowing. (Clearly, though, there can be shoddy applications of the comparative method, and there can be careful applications of lexicostatistics which seek to identify borrowings.) In the present case, it is more than likely that Doura has borrowed from Motu, since speakers of the latter have long been prolific traders and have had a certain cultural dominance.

Although lexicostatisticians occasionally claim to be practising the comparative method, e.g. Dyen *et al.* (1992), there can be a radical difference between the family trees attained by the two methods. Other discussions of differences in the outcomes of lexicostatistics and the comparative method are: for Australian languages Dixon (1980: 254; 1990: 399–401) and Johnson (1990: 430–2), and for Austronesian languages Blust (1990: 146–8).

The rather complex history of the West CP linkage shown in Figure 6.2 is the result of the recursive application of the comparative method as illustrated above. It would require a small monograph to detail and exemplify all the sound changes and other innovations which characterize each stage in the progressive break-up of this linkage, but the most significant of the shared sound changes involved in the detection of each stage are set out in Figure 6.4. These sound changes are inferred from the sound correspondences in the Appendix and are discussed in Ross (1994b).

The sequencing of sound changes plays an important part in the construction of Figure 6.4. In the box immediately above the Nuclear West CP (NWCP) linkage are the following three sound changes:

PCP ⋆t > NWCP ⋆k
PCP ⋆d > NWCP ⋆t
PCP ⋆r > NWCP ⋆d

It should be evident that the changes must be posited in this sequence. If the second change, PCP ⋆*d* > ⋆*t*, had occurred *before* the first change, PCP ⋆*t* > ⋆*k*, then PCP ⋆*d* would have merged with ⋆*t* to become NWCP ⋆*k*. But this did not occur.

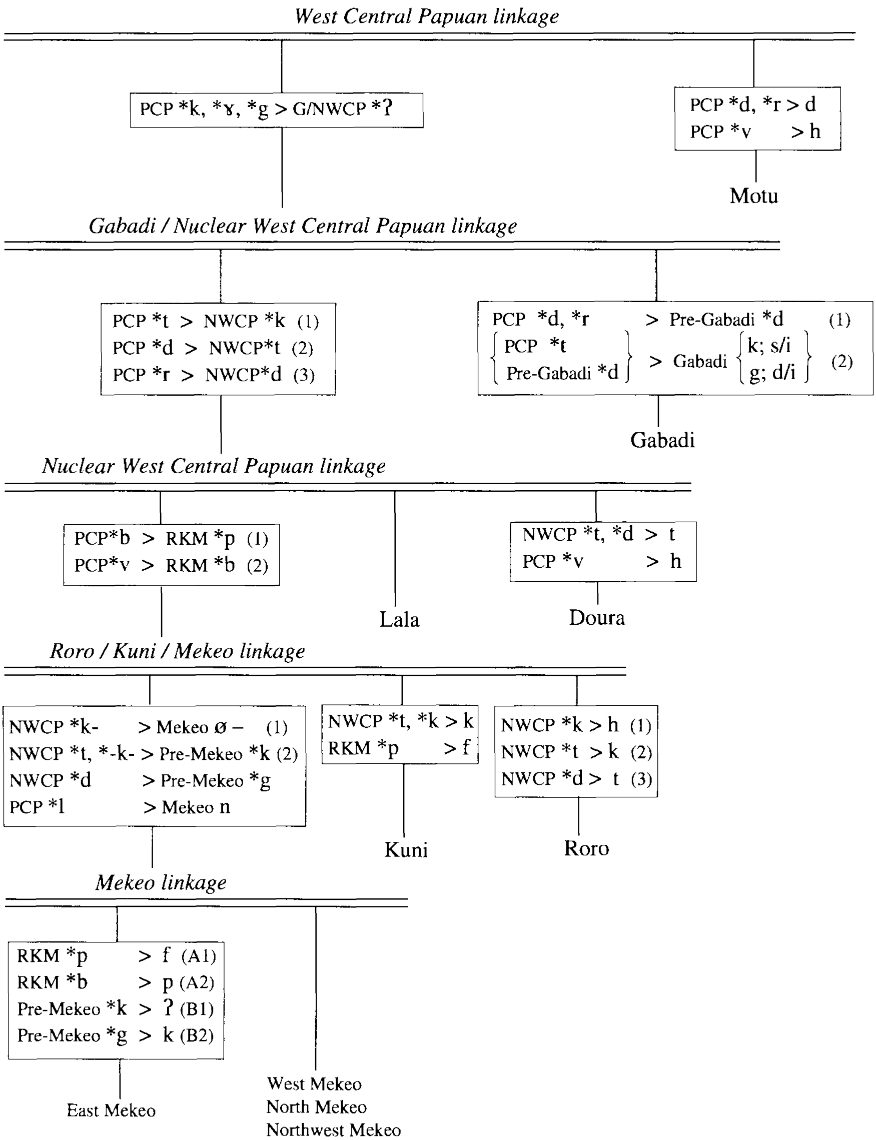

Figure 6.4 Major sound changes in West Central Papuan languages

Sequencing of this kind plays an important role in enabling us to sort out the complex web of sound changes which have occurred during the development of the West CP languages. There are cases in Figure 6.4 where the same change occurs in more than one language, but the exigencies of sound change ordering show that we are dealing with parallel changes, not with shared inheritance. For example, the backing of NWCP **t* to *k* is reflected

in Roro, in Kuni, and in Mekeo lects. It would be neater to attribute it as a single change to the Roro/Kuni/Mekeo linkage, but we cannot do this because in Roro it is preceded by NWCP *k > h, in Kuni it results in merger of NWCP *t and *k, and in Mekeo it is preceded by NWCP *k- > Mekeo ″.

It would probably be wrong to describe these as *independent* parallel changes. Where a similar change occurs in neighbouring communalects we may suspect that a sound change has diffused across the boundaries between them as a result of contact and bilingualism. It is partly because of this that the history of the West CP linkage is reconstructed as the progressive break-up of a lectal linkage rather than as a sequence of simple separations.

Language contact

The second kind of community event, which entails contact between communities speaking different lects, is not readily represented in a tree diagram and I have not attempted to present such events in Figure 6.2. However, as I shall indicate below, one such event, affecting the Nuclear West CP linkage, can be clearly identified.

A caveat probably needs to be made in the present context with regard to community events entailing language contact. In recent times there have been claims in the linguistic literature that all language change is the result of language contact, i.e. that no language change occurs without language contact and that the only kind of community event relevant to language change is the second kind (e.g. Bailey and Harris 1985). While it is undoubtedly true that much language change results from contact, and that linguists employing the comparative method have often underestimated or ignored such effects (Ross 1996), the claim that community events of the first kind do not cause language change is unsubstantiated. On the contrary, a case like the diversification of the Polynesian languages – rare though it may be – provides an instantiation of a sequence of changes resulting from separation.

An associated claim is that all language change resulting from contact entails 'pidginization' or 'creolization'. This claim misses an important distinction between the formation of a pidgin and other kinds of contact-induced changes. Most kinds of language contact cause changes in an existing speech tradition, such that the language changes, but there is no break in the tradition itself, which continues to be passed on from one generation to another within the speech community. A pidgin, however, represents the birth of a new language. Thus Pacific Pidgin originated when Melanesian labourers were taken to work on colonial plantations in the nineteenth century, creating brand new speech communities formed from Melanesians speaking different Oceanic languages and their English-speaking overseers. Pacific Pidgin was created out of the communicative efforts of the members of these new communities, separating and developing into the modern varieties of Pacific Pidgin spoken in Papua New Guinea, the Solomons, Vanuatu and various parts of Australia. Such a new birth entails linguistic beginnings and stages of growth which

are specific to new pidgins: they do not occur in other kinds of contact-induced language change where there is continuity of tradition.

The reconstructible morphological history of the Central Papuan languages indicates that none of the community events in the history of the CP languages has entailed the birth of a new language, but contact has certainly been frequent. The easternmost of recorded CP languages, Ouma, Magori, Yoba and Bina, show the effects of their speakers' bilingualism in at least one non-Austronesian language, and when they were surveyed were rapidly being replaced by non-Austronesian languages (Dutton 1976, 1982; see also Volume III). Off the coast between Ouma and Keapara is Mailu Island, whose inhabitants now speak a dialect of the non-Austronesian language Mailu, but there is evidence that they once spoke an Oceanic language, presumably of the CP group (Dutton 1982: 258). Motu speakers around Port Moresby live in close contact with speakers of non-Austronesian Koita or Koiari and the latter often speak Motu (Dutton 1994). These, however, are events which have occurred in the recent past or are occurring at present.

Figure 6.4 shows the relationship between a set of linguistic events – the reconstructed sound changes – and the sequence of community events inferred from them, i.e. the break-up of the linkage. There is one reconstructible linguistic event, however, which is not shown in Figure 6.4 because it entails language contact rather than language fissure.

The vocabularies of West CP languages contain a number of words which are apparently of PT origin, but whose forms indicate that they are borrowings from a language or languages outside the CP group. A detailed analysis of these words is presented in Ross (1994b) and will not be recapitulated here. Three points of present interest emerge from that analysis. First, the borrowings are biased in favour of items and activities associated with the sea: they include 'octopus', 'squid', 'shark', various kinds of fish, 'tern', 'salt', 'reef', 'fish hook', 'bailer', and 'anchor'. Second, the forms of a number of these words indicate that they were borrowed from a language belonging to an earlier phase of the Are/Taupota linkage. Proto-PT voiced stops are devoiced in Are/Taupota languages, but not in CP languages; Proto-PT final consonants are retained with a paragogic vowel in Are/Taupota languages, but are lost in CP languages. The present-day Are/Taupota languages are spread along the north coast of Papua (see Figure 6.1). Third, where loanwords would have been eligible to undergo the sound changes which mark the split of the Nuclear West CP linkage from Gabadi (the changes listed above), they did not undergo them; they were affected, however, by subsequent changes. These observations tell us that there was an influx of loanwords into Nuclear West CP, the linkage of lects ancestral to Doura, Lala, Roro, Kuni and the Mekeo linkage, and that the source of this influx was the languages of a group living probably on the north coast of Papua, whose lifestyle was more oriented toward marine activities than was the lifestyle of Nuclear West CP speakers. (Some such loans also occur in other CP languages, and we may infer – but cannot demonstrate – that they occurred at the same time.)

A sequence of community events

Most of the community events that can be inferred from the linguistic events reconstructed here have already been mentioned above. My purpose here is to draw them together into a sequence, i.e. a sort of skeleton narrative. I shall ignore the Ouma/Magori languages, because there are simply not enough relevant data to let us detail their relationship(s) to the other CP languages. This lack is due partly to their moribund state, partly to the effects of contact (about which we do know quite a lot, thanks to Dutton's 1982 work; see also Dutton in Volume III).

The community events which we can infer are as follows:

1 A group of people speaking a language ancestral to PCP became separated, geographically and socially, from other PT speakers. Precisely who these PT speakers were, we cannot say. The island languages of Misima, Nimoa and Sudest are somewhat more closely related to the CP group than are other PT languages, but that relationship is not particularly striking, and it is probable that there has been a good deal of population movement in the southeast Papuan region since the departure of pre-PCP speakers, obliterating the kind of evidence which would allow us to uncover the origins of pre-PCP more exactly.
2 Pre-PCP speakers stayed close enough together for long enough for the innovations reflected in all CP languages to occur, i.e. for pre-PCP to become PCP (it is possible, of course, that some of the innovations predated their separation from other PT speakers).
3 On the interpretation in Figure 6.3, PCP diversified into a lectal linkage – probably a chain of lects spread along the coast. Since we cannot infer that there was a discrete Proto-Sinagoro/Keapara, we are left with the alternative inference, that the Sinagoro/Keapara linkage is the present-day manifestation of part of the original CP lectal linkage. This in turn implies that the Sinagoro/Keapara area was (or was part of) the 'homeland' of the CP lectal linkage. This inference is supported by the conservatism of the Sinagoro lects (whose present-day forms resemble reconstructed PCP forms quite closely), since conservatism reflects an absence of the social upheaval associated with migration (Ross 1991).
4 Speakers of CP lects moved westwards to occupy the areas in which the West CP languages are now situated. The fact that the linkage broke up slowly suggests a generally steady diversification. The fact that the languages manifest more innovations the further west one goes, implies that movement was basically from east to west (Ross 1991). The present-day distribution of the languages suggests that there were also constant attempts to penetrate inland, some of which were obviously successful.
5 At a certain point during this east-to-west movement, CP speakers were in fairly intense contact with a PT-speaking group, probably from the north Papuan coast. This event occurred after Motu and Gabadi had

become separate languages but while the Nuclear West CP linkage was still intact. Since the loanwords acquired from this north-coast PT language tend to be associated with the sea, it is a reasonable inference that its speakers travelled from their homeland to Central Papua and that contact occurred there. We may also infer that some of the new arrivals settled in Central Papua with or near speakers of Nuclear West CP lects.

6 After the integration of the new arrivals, the Nuclear West CP linkage continued to disintegrate into separate languages, as shown in Figure 6.2. However, when the ethnographic record of CP speaking groups begins in the 1870s, it is clear that these groups (with the exception of Ouma/Magori) were still in regular trading contact, and we may infer that this contact continued a situation which had prevailed throughout the history of the CP group.

DATING LINGUISTIC EVENTS: THE INTERFACE WITH ARCHAEOLOGY

In the previous section I have illustrated the claim that, using the comparative method, linguists can reconstruct a sequence of linguistic events. However, the method does not allow linguists to date these events. For this they must turn to archaeologist colleagues.

How should we handle the interface between linguistics and archaeology? An important part of the answer is a negative: the interface should not be attempted before the methodologies of the two disciplines have been fully applied. The data of linguistics and the data of archaeology are radically different in kind and are processed by completely different methodologies. It is decidedly illegitimate, for example, for a linguist to take archaeological findings into account in arguing for a sub-grouping hypothesis. However, once the different methodologies of the two disciplines have been applied, a positive commonality emerges: both disciplines produce sequences of events, the one linguistic, the other material-cultural, and these two kinds of event sequence can be related to each other. Indeed, since both kinds of reconstructed events are manifestations of change in human societies, it would be irrational not to seek a relationship.

The first practical question which arises when we seek this relationship is: which sequence of linguistic events, if any, matches which sequence of material-cultural events? The only empirically verifiable point of interface between the two kinds of sequence lies, of course, in the present or the historically known past. In the case of CP, at least one interface point is accessible to us. There is an archaeologically reconstructible sequence of material-cultural events which terminates with the present-day Motu, who speak a present-day CP language. I must also admit, however, that in the present case interfacing is simplified by the fact that there is really no competing sequence of material-cultural events in the CP speaking area, and

it seems that much of the area (which was presumably malarial) may have been uninhabited when the ancestors of present-day CP speakers arrived. It is interesting to note that CP speakers seem not to have occupied the coast much further west than the present extent of the Roro language. It is in this area further west, around the Kikori Delta, that evidence of earlier coastal habitation (at least since 1000 BC) is found (Rhoads 1982).

There are at least two other CP areas of interface between linguistic events and material-cultural events, one involving the Roro, the other the Mailu. But the archaeological record as yet provides insufficient continuity for the former, whilst the linguistic record is discontinuous for the latter, as Mailu is a non-Austronesian language.

The second practical question is: how complete and how well attested are the event sequences that we are seeking to match? We have already seen that the linguistic sequence we have reconstructed almost certainly has holes in it for the eastern end of the CP area: the languages of the Ouma/Magori group are moribund and render up insufficient relevant data, while the Mailu, who now speak a non-Austronesian language, appear 'culturally CP', implying perhaps that at least one CP language has become extinct in the past few centuries (Dutton, Volume III). The reconstructed material-cultural event sequences also have holes in them: the sites for which sequences have been reconstructed all lie in the areas occupied by present-day Motu and Roro speakers, i.e. in the centre and west of the CP area. What is perhaps fortunate is that the holes in both sequences are associated with the same geographic region, leaving us with reasonably well found linguistic and material-cultural event sequences in the middle and west of the CP area.

The most important archaeological work has been done on Yule Island (where Roro is now spoken) (Vanderwal 1973) and at the Nebira sites some 15 km inland from Port Moresby (in an area where the non-Austronesian Koita language is spoken) (Allen 1972). Some work has also been done at coastal sites in the Motu-speaking area around Port Moresby: Motupore, Taurama, Boera and Eriama (Bulmer 1971, 1982; Swadling 1980a). These sites represent similar cultural sequences and, although different scholars have used different terms for the various periods at different sites, they can be integrated into a single material-cultural sequence (all dates, obviously, are approximate; the terms are mainly from Allen 1977a, 1977b, 1977c):

1 Early Ceramic Horizon: 100 BC to AD 1000
   (a) Initial Ceramic Phase: 100 BC to AD 1, represented at Oposisi (Yule Island) and Nebira 4
   (b) Developmental Phase: AD 1 to 1000, represented by the Ravao culture on Yule Island and Red Slip ceramics at other sites
2 Massim Intrusion: AD 1000 to 1200, represented by the Eriama culture
3 Later Ceramic Horizon: AD 1200 to 1870
   (a) Middle Period: AD 1200 to 1650, represented by Urourina culture on Yule Island and Taurama shell-and-comb decorated pottery

(b) Late Period: AD 1650 to 1870, represented by Taurama incised-punctate pottery, which is synonymous with ethnographic Motu ware.

The culture of the Initial Ceramic Phase was evidently intrusive, arriving with an already developed pottery-making tradition with clear antecedents in Lapita (to which I return on p. 162) (Bellwood 1978: 255–8), and maintaining trade links with the islands off the north coast of Papua, as Fergusson Island obsidian was found in the Oposisi assemblage. Vanderwal (1973) feels that the pottery of the Ravao culture has possible links to the Collingwood Bay/northern Massim area, i.e. the north coast of Papua and its offshore islands. Allen (1977a) and Bulmer (1979) both indicate that the culture of the Developmental Phase is represented both at immediately coastal sites and at sites just inland.

The Massim Intrusion period, as its name suggests, was associated with the arrival of people from the north coast of Papua or its offshore islands. It was a period of upheaval, but the upheaval is attributable not just to the arrival of outsiders, but to two other factors. The first was internal pressure. Allen (1977b) suggests that the enormous cultural province of the Developmental Phase was starting to break up, with the growth of central villages (Hanuabada at Port Moresby, Maopa and Mailu) which had significantly greater populations than other villages, a potting industry, and served as nodal points connecting otherwise discrete trading systems (see also Irwin 1974). The second factor was population pressure from inland peoples. At this time people living at inland sites of the Developmental Phase culture were apparently displaced by new arrivals from inland, an inference confirmed by oral history (Dutton 1969; Gaigo 1979; Swadling 1980b). Bulmer (1971) concludes that the inland Nebira 2 village changed hands during this period.

Although the Massim intruders undoubtedly contributed to the changes which occurred at this period, one may well ask whether their arrival was not itself in some way the result of the other two factors, the more so as there is evidence of trading contact between the northern Massim and west central Papua over the preceding millennium. Whatever the answer, there is some disagreement in the archaeological literature as to whether the intruders are the main ancestors, at least culturally, of the present-day Roro and Motu (Vanderwal 1973; Allen 1977a; Bulmer 1979, 1982) or whether the intruders simply contributed a relatively superficial cultural change (Bellwood 1978: 269–70; Swadling 1980a, 1980b).

The Late Ceramic Horizon is less controversial, and entails continuity from around AD 1200 to the present day.

It is fairly clear that this material-cultural event sequence at best matches only Steps 4, 5 and 6 of the linguistic event sequence reconstructed at the end of the previous section. This matching is summarized in Table 6.5. The first three steps of the linguistic event sequence concern a period before the settlement of the West CP area. This settlement is represented archaeologically

Table 6.5 Matching linguistically and archaeologically inferred event sequences in Central Papua

Linguistically inferred community events		*Archaeologically inferred material-cultural events*	
1	Pre-PCP separates from other PT languages		(none)
2	Pre-PCP becomes PCP		(none)
3	PCP diversifies into a lectal linkage, probably around the Sinagoro/Keapara area		(none)
4	Speakers of CP lects move westward, the resulting West CP linkage gradually breaking down, with Pre-Motu and Pre-Gabadi becoming separate languages	1	Early Ceramic Horizon: 100 BC to AD 1000
5	CP speakers are in fairly intense contact with a non-CP group of PT speakers	2	Massim Intrusion: AD 1000 to 1200, represented by the Eriama culture
6	The Nuclear West CP linkage continues to break down into Doura, Lala, Roro, Kuni and the Mekeo linkage	3	Later Ceramic Horizon: AD 1200 to 1870

by the Initial Ceramic Phase from about 100 BC. It follows from this that if archaeological correlates of Steps 1, 2 and 3 (the arrival of people speaking a language ancestral to PCP, its development into PCP and the latter's subsequent diversification into a lectal linkage) are ever found (and I would expect them to be found in the present Sinagoro/Keapara area) then dating would place them earlier than 100 BC.

Steps 4, 5 and 6 match the material-cultural event sequence rather well. Working back from the common anchor-point provided by the present, we see that Step 6 corresponds with the Later Ceramic Horizon, Step 5 (with its arrival of a PT speaking group) matches the putative Massim Intrusion quite strikingly, while Step 4 corresponds with the Early Ceramic Horizon. The matching of Step 5 with the Massim Intrusion also contributes to the resolution of the controversy as to whether the people of the Early Ceramic Horizon or the Massim intruders were the principal ancestors of the present-day Roro and Motu. The linguistic evidence shows continuity from the period before the intrusion, i.e. from the Early Ceramic Horizon, with some loanwords reflecting the intrusion itself. Since language change is normally a good reflex of cultural change, it seems that Bellwood and Swadling are correct in emphasizing a fundamental continuity between the Early and Later Ceramic Horizons.

The matching of linguistic and material-cultural event sequences in turn allows us to put tentative dates on the genetic ('family') tree in Figure 6.2. It implies that the initial diversification of the West CP linkage had occurred

by 100 BC; and that Motu and Gabadi had separated from the linkage before the Massim Intrusion affected the Nuclear West CP linkage around 1000 AD.

CONCLUSION

Using the CP case as an example, I have attempted to make explicit some of the methods used to reconstruct a sequence of linguistic events and to show how such a sequence can be matched to a corresponding sequence of archaeologically reconstructed events. What is new here is the attempt to make these procedures more explicit. The procedures themselves have certainly been applied previously in Oceania, most notably to match the spread of Oceanic languages across the Pacific with the spread of the Lapita culture. Dempwolff (1937) and later scholars have shown that all the Austronesian languages which we now label 'Oceanic' share a set of innovations relative to Proto-Austronesian. He thereby reconstructed a set of linguistic events (the innovations) and inferred from them a community event, namely that a community speaking a language ancestral to Proto-Oceanic separated itself from other Austronesian speakers, and that after the separation their language underwent the set of innovations attributed to Proto-Oceanic (revised in the light of more recent research: see Ross 1994c). Subsequent work has led to the reconstruction of a partial event sequence for the spread of Oceanic languages, through the Solomon Islands and North/Central Vanuatu to Fiji and thence into Polynesia (for surveys of the literature, see Ross 1988, 1994c; Pawley and Ross 1993, 1995). It is now generally accepted that this sequence corresponds to the expansion of the Lapita culture (although the pottery characterizing Lapita itself disappears in western Polynesia) (Pawley and Green 1985; Spriggs 1995). This matching in turn allows us to associate the Proto-Oceanic 'homeland' with the Bismarck Archipelago, with New Britain as the best candidate within it (since it is the source of much of the obsidian traded with Lapita pottery), and to date the break-up of Proto-Oceanic to about 1200 BC.

It is instructive to look back at the literature prior to these matchings, as this relied on glottochronological datings to match language and material culture. Glottochronological calculations gave a date of about 3000 BC for the break-up of Proto-Oceanic (Pawley and Green 1973), leaving Bellwood (1978: 125, 244) with an insoluble conundrum: he could find no archaeological evidence corresponding to the incursion of Austronesian speakers into Melanesia supposedly prior to 3000 BC, and no linguistic reflex of the Lapita culture, which he dated to around 1500 BC. Once the glottochronological date is set aside, however, and one extrapolates backwards from the present – where we know that modern western Polynesians (Tongans and Samoans) are both the furthest-flung descendants of the Lapita potters and are speakers of Oceanic languages – it is somewhat less difficult to match the linguistic and archaeological event sequences with each other.

The examples of the interface between linguistics and archaeology which I have used here are, I know, relatively simple ones. Clearly there are a number of types of difficulties which can make the matching process more complex. On both the linguistic and the archaeological sides of the fence we often find gaps in the available data, gaps which are sometimes by definition unfillable because either the linguistic or the archaeological evidence has been wiped out. We also find cases where a number of different linguistic and cultural sequences have coexisted or followed each other in a given area. It is beyond the scope of this chapter to deal with the effects of linguistic coexistence. Suffice it to say, however, that language contact, whether it results in change in an existing language or in its replacement by another language, frequently leaves its mark on the languages themselves (Ross 1994a, 1996). It is certainly my conviction that, given sufficient relevant data, sufficiently sensitive methodological tools, and sufficient care, the linguist can delve deep into the prehistory of languages, and the linguist's prehistory can be matched with the archaeologist's prehistory just as long as both are able to deliver a sequence of events and to match them working backwards from the present or from recorded history.

APPENDIX: CONSONANT CORRESPONDENCES

The key is at the end of the Appendix

Central Papuan family

POc[a]	★p fortis	★p lenis	★b	★m	★p^w	★b^w	★m^w	★t	★r, ★R	★d, ★dr, ★j
PPT	★p	★v	★b	★m	★p^w	★b^w	★m^w	★t	★r	★d, ★j
PCP		★v	★b	★m	★p^w	★b^w	★m^w	★t	★l	★d
Bina	p	v; Ø/_u	b	m	·	·	m^o	t; s/_i	l	d
Yoba	p	v; Ø/_u	b	m	·	·	m^o	t; s/_i	r	d; d~s/_i
Magori	p	v; Ø/_u	b	m	·	·	m^o	t; s/_i	r	d; t~d/_i
Ouma	p	v; Ø/_u	b	m	·	·	·	t~h~ʔ; h~s/_i	r	d
Keapara (Aroma)	p	v; Ø/a_u	p	m	·	·	m	Ø~ʔ	l	r
Keapara (Hula)	p	v; Ø/a_u	p	m	·	·	m~m^o	Ø~ʔ~k	l	r
Sinagoro (Taboro)	f	v; ɣ~Ø/o, u	b	m	·	b^o	m^o	t; s/_i, e	r	d
Sinagoro (Balawaia)	p	v; ɣ~Ø/o, u	b	m	·	b^o	m^o	t; s/_i, e	l~r	d~r
Motu	p	h	b	m	p^o	b^o	m~m^o	t; s/_i, e	r	d
Gabadi	v	v; Ø/_o, u	b	m	·	·	m^o	k; s/i	r	g; d/i
Doura	p	h	b~p	m	·	·	·	k; s/_i	r	t
Lala	p	v	b	m	p	b^o	m~m^o	k; s/_i	l	t; d/_i
Roro	·	b; Ø/a_u	p	m	·	p~p^o	m~m^o	h; h, s/_i	r	k; s/_i
Kuni	f	b	f	m	p^o	·	m^o	k; s/_i	l	k; s/_i
N.W. Mekeo	·	v; v~p/_u	p	m	·	·	m	Ø–k–	n	k
West Mekeo	·	b; b~p/_u	p	m	·	·	m	Ø–k–	n	k; j/_i

North Mekeo	b	b; b~p/_u	p	m	·	·	m	Ø–k–	n	k; ts/_i
East Mekeo	p	p; p~f/_u	f	m	p°	·	m	Ø–ʔ–	n	Ø–ʔ–; ts/_i

Note: [a] POc *pʷ, *bʷ and *mʷ sometimes lose their labialization feature in PCP and are reflected as PCP *v, *b and *m respectively.

POc	*l/_i, u	*y; *l/_a, o	*Ø–		*n, *~n	*s, *c
PPT	*l/_i, u	*y; *l/_a, o	*Ø–		*n, *~n	*s
PCP	*Ø	*y	*Y–/*_a	*Ø/i_a	*n	*r
Bina	·	y	Ø–	–Ø–	n	ʔ
Yoba	·	y	Ø–	–Ø–	n	ʔ
Magori	Ø	y–e–	Ø–	–Ø–	n	k–r~k–
Ouma	Ø	Ø–e–	Ø–	–Ø–	n	r
Keapara (Aroma)	Ø	ð–r–	ð–	–Ø–	n	r
Keapara (Hula)	Ø	Ø–r–	Ø–	–Ø–	n	r
Sinagoro (Taboro)	Ø	y	y–	–Ø–	n	d~r
Sinagoro (Balawaia)	Ø	Ø–ɣ–	Ø–	–Ø–	n	r~d
Motu	Ø	l	l–	–Ø–	n	d
Gabadi	Ø	Ø–r–	Ø–	–Ø–	n	g; d/i
Doura	Ø	r	Ø–	–Ø–	n	t
Lala	Ø	l	Ø–	–Ø–	n	d
Roro	Ø	Ø–e–	Ø–	–Ø–	n	t; s/_i, u
Kuni	Ø	y	y–	–Ø–	n; Ø/i, e_a	d
N.W. Mekeo	Ø	Ø	Ø–		n	g
West Mekeo	Ø	Ø–l–	Ø–	–d–	n	g; j/_i
North Mekeo	Ø	·	Ø–	–z–	n	g; ts/_i
East Mekeo	Ø	l	l–	–s– [a]	n	k

Note: [a] In East Mekeo also in the environment /i_o.

POc	★k fortis	★k, ★q lenis	★g	★ŋ	·	·	★w
PPT	★k	★q	★g	★ŋ	★k^w	★g^w	★w
PCP	★k	★ɣ	★g	★ŋ	★k^w	★g^w	★w
Bina	k	ʔ~∅	g	∅; n/_i	·	∅–	v~w
Yoba	k	ʔ~∅	g	∅; n/_i	·	·	v~w
Magori	ʔ~k~∅–ʔ–	ʔ~g	g	∅	·	g^o	v
Ouma	ʔ~k~∅–	ʔ~g; ∅–	g	ʔ; n/_i	·	g^o	v~∅
Keapara (Aroma)	∅–ʔ–	ø/_i, u; ɣ/_e, a, o	k	ɣ	w–	k^w	w
Keapara (Hula)	k~∅	ɣ/_i, e, u; ∅/_a, o	k	ɣ	k^w–	k^w–	w
Sinagoro (T)	k	ɣ	g	ɣ; ∅–	k^w–	g^w	v
Sinagoro (B)	k	ɣ	ɣ~g	ɣ	k^w	$ɣ^w$	w~v
Motu	k	∅~ɣ	g	∅~ɣ	k^w	g^w	v
Gabadi	ʔ~∅	∅~ʔ	ʔ~∅	∅~n	$\emptyset^o$–	$\emptyset^o$–$ʔ^o$–	v~u
Doura	ʔ~∅	∅	∅~ʔ	∅~n	·	$\emptyset^o$–	v
Lala	∅	∅	∅	∅~n	v–	$\emptyset^o$–∅–	v
Roro	∅~ʔ	∅~ʔ	∅~ʔ	∅~n	∅–	∅–	w~b
Kuni	∅	∅	∅	∅	$\emptyset^o$–v–	–v–	v
N.W. Mekeo	∅	∅	∅	n~∅	$\emptyset^o$	$\emptyset^o$	$\emptyset^o$~w
West Mekeo	∅	∅	∅	n~∅	$\emptyset^o$	w	w
North Mekeo	∅	∅	∅	n~∅	$\emptyset^o$	$\emptyset^o$~w	w~v
East Mekeo	∅–ʔ–	∅–ʔ–	∅	n~∅	$\emptyset^o$–$ʔ^o$–	$\emptyset^o$–f–	v~f

Other Papuan Tip languages

POc	*p fortis	*p lenis	*b	*m	*p^{w}	*b^{w}	*m^{w}	*t	*r, *R	*d, *dr	*j
PPT	*p	*v	*b	*m	*p^{w}	*b^{w}	*m^{w}	*t	*r	*d	*j
Maisin	f	v-Ø; w/_u; ø/o_	f (b)	m	f	b^{o}	m^{o}-w-	t	r	t (d)	t
Ubir	f	b; Ø/_u	f (b)	m	f^{w}	f^{o}	m^{o}	t; s/_i	r	t; h/_i	·
Gapapaiwa	p	v; Ø–/_u	p (b)	m	p^{o}	(b/^{U})	m^{o}	t; s/_i	r	t; s/_i (d)	·
Wedau	p	v; Ø/_u	p (b)	m	p^{o}	(b^{o}; b/^{U})	m^{o}	t; ɣ~Ø/_i	r	t; Ø/_i (d)	t (d)
Tawala	p	w; w/_u	p (b)	m	p^{o}	p^{o} (b^{o}; b/^{U})	m^{o}	t; h/_i	l	t; h/_i (d)	(d)
Bwaidoga	f	v; Ø, ɣ/_o, u	b	m	f^{o}	b^{w}	m^{o}	t; ɣ/_i; s/_e	l	d	·
Iduna	f	v; Ø/_u; w/_o	b	m	f^{w}~f^{o}	b^{o}	m^{o}	t; h/_i; s/_e	l	d	d
Kalokalo	f	v; Ø/_u; g/_o	b	m	f^{w}~f^{o}	b^{w}~b^{o}	m^{w}	t; Ø/_i; .	l	d	·
Dobu	p	Ø	b	m	p^{w}	b^{w}; b/^{U}	m^{w}	t; s/_i	l	d	d
Duau	p	h; w/_o	b	m	p^{w}	b^{w}	m^{w}	t; s/_i	l	d	d
Tubetube	p	Ø; w–/_o	b	m	p^{w}~p^{o}	b^{w}~b^{o}	m^{w}~m^{o}~m	t; t~s/_i	l	t~d–l–	–l–
Suau (Kwato)	p	h; w–/_o	b	m	p^{o}	b^{o}	m^{o}	t; s/_i	l	d	d
Kilivila	p	v; Ø/_u; w–/_o; –Ø	b	m	p^{w}~p^{o}	b^{w}~b^{o}	m^{w}	t; s/_i	y; –Ø (l)	d; s/_i	d~s
Sudest	b	v; Ø/_o, u	b	m	·	b^{w}	m^{w}	r; t–	y; .	d	j

POc	★l	★y	★n, ★~n	★~n	★s, ★c	
PPT	★l	★y	★n	★~n	★s	★s
					fortis	lenis
Maisin	r	∅–, –y–	n	n	·	s~∅
Ubir	n; n~∅–_i	y	n	n	g	s~∅
Gapapaiwa	n; ∅/_i	y–y~l–	n	·	g	s~∅
Wedau	n; ∅/_i	ð	n	·	ɣ	∅
Tawala	n	y	n		g	h~∅
Bwaidoga	n	y	n	n^{i}	ɣ	∅ (s)
Iduna	n	y	n	n^{i}	g~y	∅ (s)
Kalokalo	n	y	n	l^{i}	g	∅ (s)
Dobu	n; ∅/_i; l/_★$^{\#}$	y	n	n^{i}	s	s
Duau	n; ∅–_i; l/_★$^{\#}$	y	n	n^{i}	s	s
Tubetube	l	y	n	n^{i}	s	s
Suau (Kwato)	l	y	n; ∅/_i	·	s	s
Kilivila	l; ∅/_i	y	n~l	n^{i}	l	s
Sudest	l	·	·	~n	∅	ð–ð–∅

POc	*k fortis	*k, *q lenis		*g	*ŋ	·	·	*w
PPT	*k	*q–	*–q–	*g	*ŋ	*k^{w}	*g^{w}	*w
Maisin	k–∅–	∅~k–	–∅–	·	–∅–	k–	·	v; –∅
Ubir	k–∅–	∅–	–∅–	g–∅–	–n–	k^{w}~k^{o}–	·	w; –u~b
Gapapaiwa	k	k–	–∅–	k (g)	–n–	k^{w}–	·	w
Wedau	k–∅–	∅–	–∅~ɣ–	–∅– (g)	–ɣ–	k~∅^{o}–u	·	w
Tawala	k–∅–	∅–	–∅~h–	–∅– (g)	–g–	∅^{o}~k^{o}–	g^{w}–	w
Bwaidoga	k	∅~ʔ~k–	–ɣ~y–	g	–ɣ–	k^{w}–; w~k^{w}/_*#	·	w
Iduna	k–ɣ–	∅~ʔ~k–	–∅~y–	g	–y–; ∅/_o	· ; w/_*#	g^{w}–	w
Kalokalo	–k–	k–	–∅~ɣ~g–	g	–g–	k^{w}–; w/_*#	g^{w}–	w
Dobu	k~ʔ–ʔ–	ʔ~∅~k–	–∅–	g	–∅–	k^{w}–; w/_*#	g^{w}–	w; –u
Duau	k–	k~ʔ–	–∅~h–	g	–∅–	k^{w}~k^{o}–; k/_*#	g–	w
Tubetube	k	k–	–∅–	g	–n–; ∅/_–i	∅^{o}–	·	w
Suau (Kwato)	k–ʔ–	ʔ~∅–	–h~∅–	g	–n–; ∅/_–i	∅^{o}~w–	g^{o}–	w
Kilivila	k	k–	-∅–	g	–g–	k	g–	w
Sudest	·	ɣ–	-∅–	g	·	·	g–	w; –u

Key

Orthographies are as in Ross (1988). The terms 'fortis' and 'lenis' are used as in Ross (1988).

x–x–x	refers to position in word (i.e. *initial–medial–final*)	x^{o}	*x* + rounding of the following vowel
x; y/_a	*y* in the environment */_a*, but *x* elsewhere	x^{i}	*x* + raising of the following vowel
/_a, b	both */_a* and */_b*	*x~y:*	either *x* or *y*, with no known conditioning
s/i	both *s/_i* and *s/i_*	$/^{U}$	in an unstressed syllable
(x)	reflex frequently occurring in borrowed words		

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NOTES

1 Of course, it is possible for two community events of this kind to occur without any innovations occurring in the language during the intervening period. If this had been the case here, then we would have no evidence for the existence of Proto-CP. However, since community events like separation cause social upheaval, we infer that this is usually reflected in language change, i.e. in system events.
2 There are exceptions to this tenet. Certain sound changes occur by lexical diffusion. That is, they affect eligible words (words containing the relevant sound) one by one, rather than affecting all eligible words simultaneously. This means that they are regular but not exceptionless. The key paper on this topic is Labov (1981). Ross and Durie (1996) provide a discussion.
3 The innovations characterizing PCP relative to Proto-Oceanic were analysed by Pawley (1975), reanalysed by Lynch (1983) on the basis of a larger database, and then analysed relative to PPT by Ross (1994b).
4 If the sound change is one which occurs by lexical diffusion, then it may affect more lexical items in some dialects than in others. The classic description of this in Oceania is by Lincoln (1973).

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7 *The enigma of Pama-Nyungan expansion in Australia*

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INTRODUCTION

The Pama-Nyungan family of languages covered the seven-eighths of the Australian continent throughout the east, centre and south; a number of non-Pama-Nyungan languages belonging to language families more distantly related to Pama-Nyungan occupy the extreme north-central zone of the continent. A somewhat oversimplified diagram of the linguogenetic relationships relevant to this paper is in Figure 7.1.

The Pama-Nyungan family has an apparent age, from rough linguistic comparison, of around 3,000–5,000 years, putting its expansion in the Mid-Holocene period. In this period also, the 'small-tool tradition' arose in Australia, and the distribution of unifacial tools in particular coincides with Pama-Nyungan distribution. A number of economic and social processes referred to as 'intensification' and widening of social and trade networks are also evident in the archaeological record at that time.

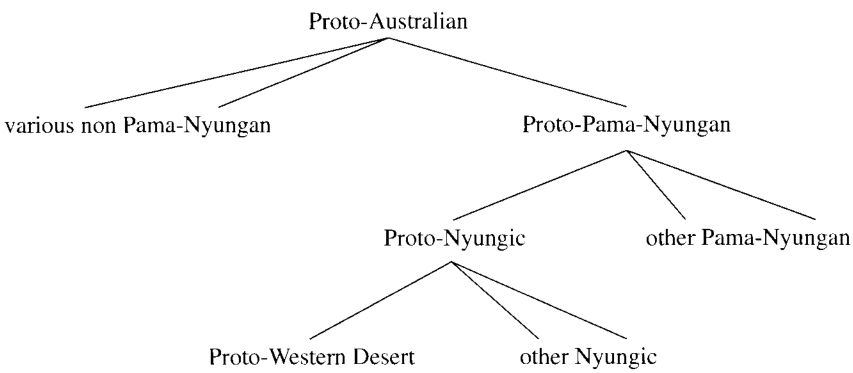

Figure 7.1 Genetic relations of Australian languages (simplified)

Mid-Holocene language expansions in other parts of the world have been attributed exclusively to agricultural expansion by Renfrew and Bellwood, who see Pama-Nyungan expansion as a problem for this theory. Nichols (Ch. 10, this volume) also emphasizes the 'Neolithic' associations of some language spreads, but her concept of a 'spread zone' is not so tied to a particular mode of production, and more applicable to Pama-Nyungan, but it provides little motivation for why a particular language expands at a particular time. In an era when the Renfrew–Bellwood view is so widely accepted as the dominant model of language spread, we see it as important to develop models of language expansion that do not necessarily depend on agricultural spread, but which do include explanation in terms of socio-cultural change.

In this chapter we present a model of Pama-Nyungan expansion which shows how the language expansion process can occur with hunter-gatherers under particular conditions. We establish first that similarity between Pama-Nyungan languages is due to their descent from a single proto-language, not in any significant way by diffusional convergence processes. Genetic distance and other biological evidence (e.g. distribution of the retro-virus HTLV1) tends to support the idea that the expansion of Pama-Nyungan was associated at some level with the movement of people.

Linguistic and other evidence suggests that this movement was in two phases:

1 involving intense interaction with peoples already in occupation, including in favourable environments along the eastern coast
2 involving much less interaction with existing populations, a phase confined to the inland and in the central-south and west, where the pre-existing population was sparse.

Especially in the first phase of Pama-Nyungan expansion into established populations, expansion would have involved a mixture of demic diffusion and socio-linguistic propagation as a lingua franca. This would have been driven by the association of Pama-Nyungan with a more open culture including outward-reaching social alliances, cemented by broad inter-ethnic social categorization systems like sections, and large-scale ceremonial gatherings that could be supported by advances in food-gathering techniques, probably associated with greater emphasis on patrilineal descent. At a later stage, and over a narrower region, the second phase operated. Virtually all language expansion would have been accompanied by major population influxes as Pama-Nyungan speakers moved into sparsely settled areas which had been partially or fully abandoned during arid climatic periods.

LINGUISTIC EVIDENCE FOR PAMA-NYUNGAN SPREAD

The languages of Australia show a very uneven distribution of diversity. Of the twenty-eight families recognized by O'Grady *et al.* (1966), twenty-seven

are crammed into the northwestern eighth of the continent, while the remaining seven-eighths is occupied solely by languages of the 'Pama-Nyungan' family (named after the words for 'person' in the northeast and southwest extremities). Although subsequent work has redrawn the boundaries slightly, and is steadily reducing the number of non-Pama-Nyungan families (to around twelve or so, on the most 'lumpist' assessment), the asymmetry between the diverse linguistic picture in Arnhem Land and the Kimberleys, and the relative homogeneity of the rest of the continent, remains striking (see Figure 7.2).

Typical Pama-Nyungan and non-Pama-Nyungan languages differ markedly in grammatical organization. Non-Pama-Nyungan languages (e.g. Mayali or Nunggubuyu) are head-marking (i.e. predominantly marking grammatical relations on the verb), make use of both prefixes and suffixes, and have four genders; although individual languages or families may have lost one or more of these characteristics, the distribution of both types and forms is so widespread it is likely that these are ancestral. Typical Pama-Nyungan languages (e.g. Warlpiri) are dependent-marking (i.e. predominantly marking grammatical relations on the noun phrase), use suffixes only, and lack genders, and again it is likely that proto-Pama-Nyungan will reconstruct with these features. Most exceptions to this typological correlation are along the Pama-Nyungan–non-Pama-Nyungan border: the Tangkic languages are non-Pama-Nyungan but suffixing, dependent-marking and lacking gender, while Yanyuwa is Pama-Nyungan but prefixing, head-marking and with a complex gender system.

The original O'Grady *et al.* classification made use of lexicostatistical and typological criteria rather than the rigorous proofs of the comparative method, leading some scholars to question the validity of Pama-Nyungan as a genetic construct (see Dixon 1980: 255–6) and to impute its similarities to contact and typological convergence. However, there is a growing body of comparative evidence for the genetic validity of Pama-Nyungan.

Vocabulary

Hale (1982) gives a number of words widespread in Pama-Nyungan but not outside its boundaries, and O'Grady and Tryon (1990) propose hundreds of distinctive Pama-Nyungan etymologies. Although not all these etymologies are convincing, there are none the less hundreds of clear cognate sets with attestations throughout the Pama-Nyungan area and absent outside.

Pronoun sets

Blake (1988) has demonstrated the existence of two distinct pronoun sets (one Pama-Nyungan and one non-Pama-Nyungan or 'Northern') from which most modern forms can be derived (Table 7.1).

Case morphology

For Proto-Pama-Nyungan we can reconstruct distinctive ergative and locative case suffixes; in fact, some of the reconstructions attributed by Dixon (1980)

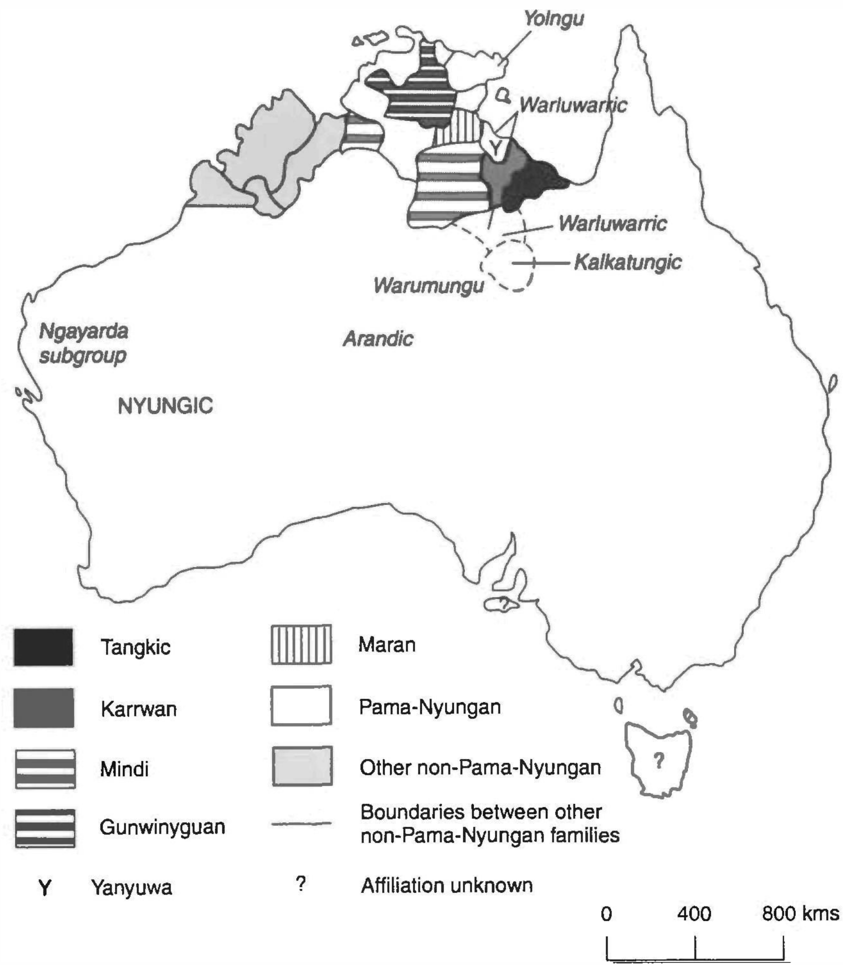

Figure 7.2 Distribution of relevant linguistic sub-groups

Table 7.1 Pronoun sets for Northern and Pama-Nyungan

Person	*Number*	*Northern*	*Pama-Nyungan*
first	singular	*ŋay	*ŋa-y
	dual	*ɲirV	*(ŋali)
	plural	*ɲirV	*ŋana
first inclusive		*ɲa-	*ŋali
second	singular	*ŋiɲ	*ŋin
	dual	*nurV / kurV	*ɲuŋpalV
	plural	*nurV / kurV	
third	singular: masc.	*nu	*ɲu
	singular: fem.	*ŋaya	*ɲan
	dual	*purV	*pula
	plural	*purV	*t̪ana

to Proto-Australian are based on forms found only in Pama-Nyungan and should therefore be treated as Proto-Pama-Nyungan reconstructions.

Verbal morphology

Again, the 'conjugation markers' imputed to 'Proto-Australian' by Dixon (1980) are based on Pama-Nyungan data alone; Alpher (1990) shows they are part of a series of verbal irregularities reconstructible for Pama-Nyungan. Other verbal affixes, such as a nominalizer -n̪t̪a ~ -ɲt̪a are widespread in Pama-Nyungan but not found outside it (Evans 1988).

Distinctive sound changes

Proto-Pama-Nyungan has been shown by Evans (1988) to merge initial apical consonants (t and n) with initial laminals (t̪ and n̪, or t̪ and ɲ, according to the language).

Summary

In sum, a suite of morphological, lexical and phonological innovations distinguish Pama-Nyungan from its closest non-Pama-Nyungan relatives along the southwestern Gulf of Carpentaria (namely languages of the Karrwan, Gunwinyguan and Tangkic families: see Evans and Jones, 1997), and although future research must address many remaining questions (e.g. spelling out how its typology came to differ so markedly from the non-Pama-Nyungan pattern, internal sub-grouping, and what cultural vocabulary can be reconstructed to Proto-Pama-Nyungan), the status of Pama-Nyungan as a genetic grouping, at a time-depth comparable to a family like Indo-European, is increasingly clear.

Within Pama-Nyungan itself, there are also strong asymmetries of distribution. Virtually the entire western half of the Pama-Nyungan region is occupied by the relatively homogeneous Nyungic group, and the plains

between the Great Dividing Range and the Darling basin also host groups occupying large areas. Other regions (such as the area southwest of the Gulf of Carpentaria, and the southeastern corner) are much more diverse. The nearest relatives of the Nyungic group are yet to be determined, but McConvell (1997) has demonstrated distinctive semantic shifts (from 'fish' to 'meat') suggesting close connections between Nyungic languages and those of the interior of Queensland and New South Wales.

To summarize the linguistic picture: over the deeply etched mosaic of linguistic differentiation, still found in the northwest and presumably once found across the whole continent, there has been superimposed the relatively homogeneous Pama-Nyungan family, whose similarities point to a Mid-Holocene expansion date. A second, more recent expansion (possibly around 2000–1000 BP) appears to have carried Nyungic speakers across the western half of the Pama-Nyungan region. Whatever led to the expansion of Pama-Nyungan, it does not appear to be linked (at least directly) to external contact, since there have been no conclusively demonstrated loanwords (*pace* O'Grady and Tryon 1990) or other linguistic influences from non-Australian languages into Proto-Pama-Nyungan.

ARCHAEOLOGICAL EVIDENCE FOR MID-HOLOCENE CHANGES IN AUSTRALIA

From the Mid-Holocene on, the archaeological record in Australia sees a quickening tempo of innovation: the appearance of more complex hafted tools (most visible archaeologically through the 'small tool tradition'), the arrival of the dingo, the exploitation of new vegetable foods such as cycad, increasing population density, more use of marginal environments, the development of new art styles, and the spreading of long-distance trade networks.

The small tool tradition

Beginning with western Arnhem Land around 5000 BP and the Western Kimberleys around 4500 BP, and then appearing right across Southern Australia by about 3,000 years ago, a range of smaller tools (microliths) appears; Tasmania was cut off from this development (Jones 1977). The distribution of backed blades, which appear in this period, corresponds almost perfectly with Pama-Nyungan distribution, and there could also be a relationship between the proposed second phase of Pama-Nyungan expansion and the distribution of tula blades. By contrast, bifacial points occupy an area in the north almost perfectly identical with the distribution of non-Pama-Nyungan languages (see Figures 7.3 and Figure 7.4, reproduced from Hiscock 1994: 269 and 274). Interestingly, the small zone of overlap between the two types (bifacial and unifacial) lies just south of the Gulf of Carpentaria, also the location of those non-Pama-Nyungan groups (Karrwan and Tangkic) most closely related to Pama-Nyungan.

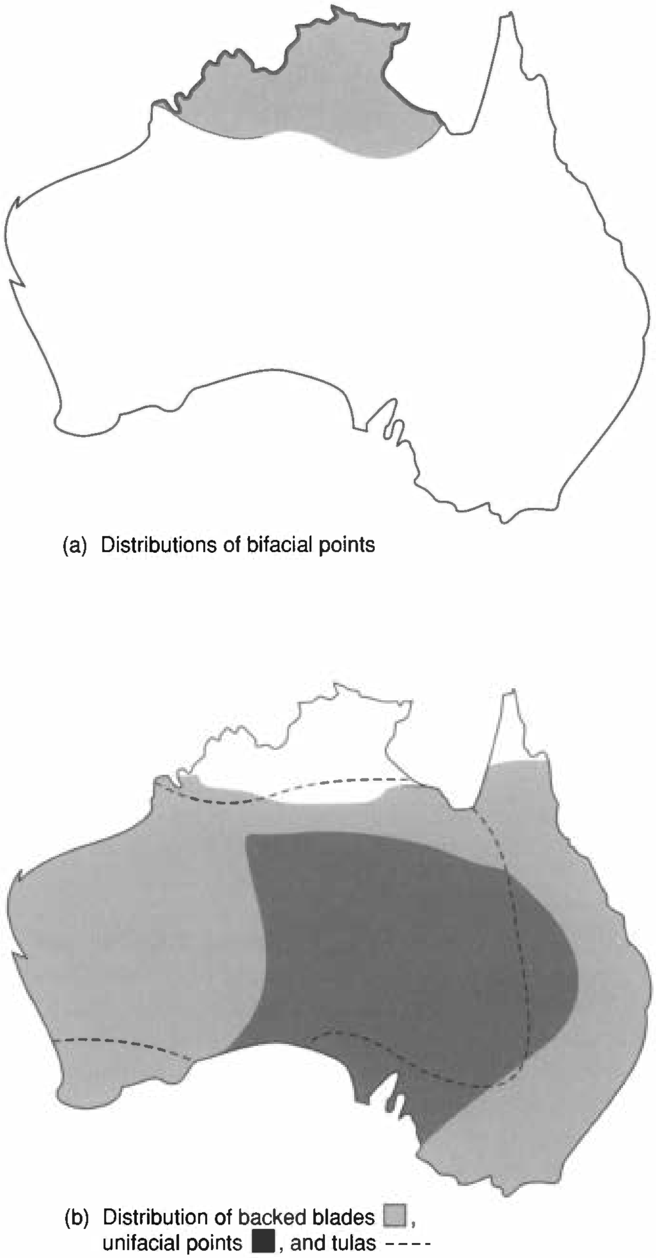

Figure 7.3 Distribution of stone tool types in Australia
Source: Hiscock 1994: 269

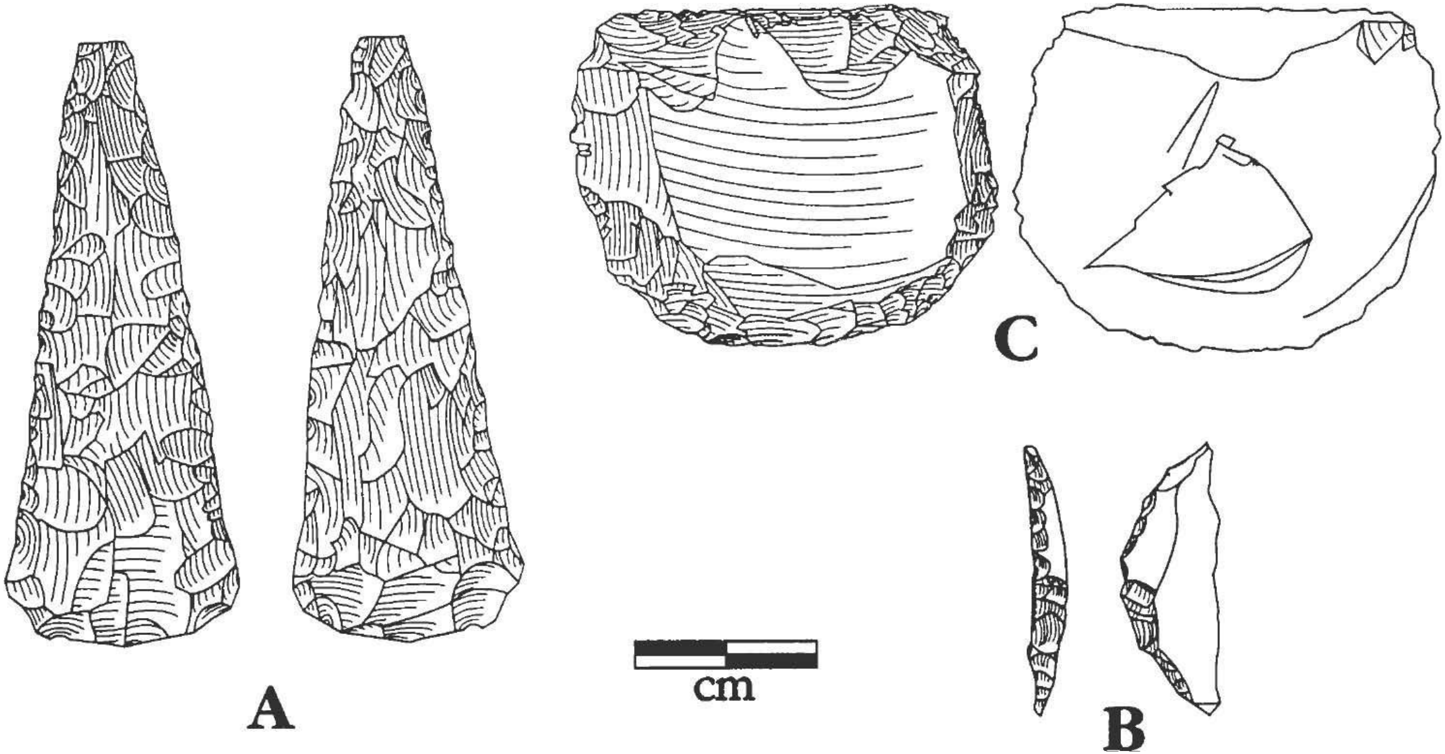

Figure 7.4 Bifacial points and backed blades: (A) a bifacial point, (B) a backed blade, (c) a tula.
Source: Hiscock 1994: 275

Vegetable foods

In many excavations the first record of the small tool tradition is synchronous with the appearance of new vegetable foods (such as cycad nuts) requiring complex preparation and capable of being accumulated for long enough to sustain large ritual gatherings (Beaton 1977). Bowdler (1981: 109), summarizing research finding Mid-Holocene developments in vegetable food exploitation, points out that 'two adaptation systems seem to have been necessary, one that provided a day-to-day staple food, another that provided a "communion food" which enabled large ritual gatherings to take place'. In arid regions seed-gathering and grinding intensifies in the Late Holocene (Smith 1986).

Population density and exploitation of marginal environments

From the Mid-Holocene on, population density increases in various areas, such as the Southern Central Queensland Uplands (5000–4000 BP: Beaton 1977), Western Victoria (5000–3000 BP: Lourandos 1985; Williams 1987). There is also growing exploitation of marginal environments (Bowdler 1981), such as the inhospitable environment of the Southern Uplands (Flood 1980) and the New England tableland region (Godwin 1997) from 4000 BP. In at least some of these areas, such as parts of the Blue Mountains (Johnson 1979: 24–38), intensification around 3000–4000 BP correlates with the appearance of the small tool tradition.

Trade networks

Comparison of the petrological features of stone tools with quarries, as pioneered by McBryde (1984), has been used to show the source of traded stone artefacts and infer the temporal development of networks of trade and ceremonial exchange. Stone material in Northeastern New South Wales appears to have been exchanged from 4000 BP on the western slopes and 1000 BP on the coast (Binns and McBryde 1972; McBryde 1977).

GENETIC AND OTHER BIOLOGICAL EVIDENCE

Language and genetics do not necessarily go together – for example many Australian Aboriginal people now speak a Germanic language, English, because of language shift. However, it is often the case that there is a correlation; for example populations speaking a remnant language also show distinctive levels of genetic markers, and language expansion based on movement of large numbers of people and intermarriage with the resident population goes along with movement of genes (Sokal 1991).

In Australia, biogenetic population studies have tended to show correlations with linguistic classification, at least on a very broad level (Kirk 1983). Pama-Nyungan language speakers of the interior cluster together as opposed to non-Pama-Nyungan speakers of the north coast. Populations contradicting this trend are the Pama-Nyungan speakers of the Yolngu group in northeast

Arnhem Land, and Cape York Peninsula, who pattern with the non-Pama-Nyungan groups. Another population formerly treated as an exception, the Lardil of Mornington Island (Tangkic group) who grouped genetically with non-Pama-Nyungan speakers despite their initial linguistic classification as Pama-Nyungan, have now been reclassified linguistically as non-Pama-Nyungan, removing the anomaly.

Another biological indicator which patterns very similarly is the retro-virus HTLV1. This is useful as an indication of biological continuity of a social group because the infection is very inefficient in its transfer, being overwhelmingly passed on only from the mother at birth or by ingestion of breast-milk by the baby. High levels are found in western Pama-Nyungan groups and very low levels in non-Pama-Nyungan groups and in northeast Arnhem Land and northern Cape York (Bastian *et al.* 1998).

The pattern which seems to be repeated here in the genetic and virological studies is one in which the correlation with linguistic classification involves a major break between western Pama-Nyungan (possibly to be equated with Nyungic or a more inclusive grouping – the data are not adequate to be certain at this point) on the one hand, and non-Pama-Nyungan and (at least some) eastern Pama-Nyungan groups on the other. This split could be explained in terms of a model in which a relatively small western peripheral group, perhaps originating in Central Queensland, were the founders of the later western Pama-Nyungan expansion and maintained their particular genetic pattern and distinctive high HTLV1 status because they moved into the arid interior, encountering relatively few resident populations at the time. The properties of this model and how it fits with the more general model of the earlier eastern Pama-Nyungan expansion are explored in the next section.

SOCIAL MECHANISMS OF LANGUAGE EXPANSION

The model proposed for Pama-Nyungan expansion is made up of at least three major waves of expansion: early western expansion, later western expansion, and recent Western Desert expansion.

Early eastern expansion

The Proto-Pama-Nyungan language, probably originating in northwestern Queensland, expanded throughout Eastern Australia around 5000–4000 BP, including into fairly densely populated coastal and riverine areas, by means of relatively small groups who introduced new forms of social organization and large-scale ritual gatherings, using this to cement marriage alliances with the previous occupants. The new language was prestigious and eventually all the populations shifted to it but elements of the older languages were retained in many areas especially where adoption of other new cultural elements was partial. The movement of Pama-Nyungan people around the Gulf of Carpentaria to occupy northeast Arnhem Land may be a late part of this phase.

Because the phenomenon of large-scale language expansion among hunter-gatherers is not a familiar one, it is worth spelling out in more detail our hypothetical (but, we believe, ethnographically plausible) scenario for how this may have occurred.[1] Recall that the three most salient changes in the archaeological record of the Late Holocene in Australia are

- the spread of microliths (the 'Australian small tool tradition') from Arnhem Land across the continent
- the appearance of large ceremonial gatherings, fed by temporary surpluses of newly utilized plant foods (e.g. cycads in some areas)
- intensification of contact and exchange.

We suggest that these three developments are linked to each other, and to the spread of Proto-Pama-Nyungan, by a shift from a social structure made of small, isolated, endogamous and inward-looking groups to one characterized by alliance, exchange, exogamy and the integration of other groups into ceremony, marriage and lithic technology. This change would have given both a motivation for language change (giving high social status to the language associated with the new, outward-looking ideology), and various social mechanisms for transmitting it, including marriage out into groups adopting the new culture, and periods of enculturation of neophyte youths undergoing lengthy ceremonial induction.

The process of constructing more complex inter-group relationships is of course an ongoing one (arguably still taking place in contemporary Aboriginal Australia), and we do not wish to suggest that it all happened at once. Indeed, many of its manifestations, such as the development of a 'sub-section' system that allows the reckoning of kinship equivalences over a vast area, are likely to have spread only in the past few centuries (McConvell 1985, 1998). None the less, there is an important sense in which the first steps in this direction were the most important ones, since it was the first establishment of inter-group trust and exchange that made the remaining elaborations possible.

To gain an idea of the magnitude of the cultural shift that would have been involved, compare the two social situations, here labelled 'isolated' and 'linked', shown in Figure 7.5.

The 'linked' structure is a simplification and essentialization of that found today; the 'isolated' structure is what we hypothesize to have been the case in areas that had yet to experience the spread of Pama-Nyungan. The isolated structure is characterized by linguistic exogamy (i.e. marrying speakers of one's own language), the organization of small ceremonies on a local basis (i.e. using only members of the group), the crucial elements of culture (songs, myths, etc.) are territorially self-contained, and technological knowledge is locally transmitted, with practically no transmission of technology from even nearby groups. Allen (1997) points out that for over two millennia the distribution of small stone points was confined to the Arnhem Land–Victoria River–Kimberleys area, with no evidence of outward spread.

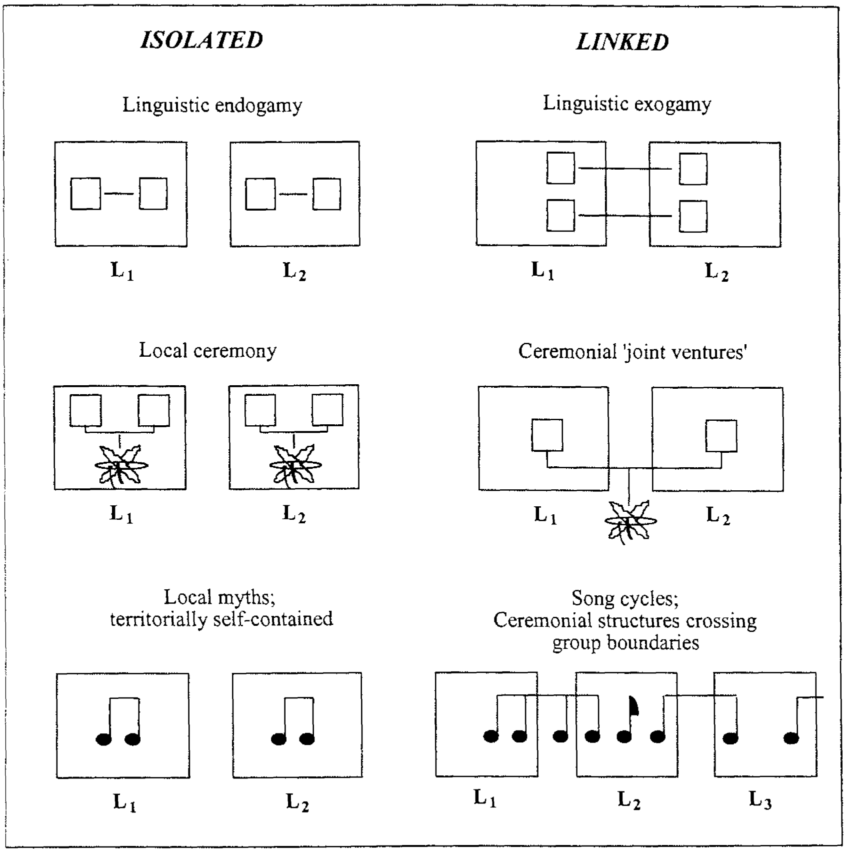

Figure 7.5 'Isolated' and 'linked' social situations

The linked structure, which we may summarize as 'diversity within unity', is characterized by linguistic exogamy (i.e. marrying speakers of another language), by the organization of ceremonies as 'joint ventures' whose staging crucially involves members of different groups (the English terms 'owner' and 'manager' are often used by Aboriginal people to explain these roles), by the development of epic cultural performances (especially the great song cycles) in which the stages of the song narrative, and the corresponding rights of performance, cross through a number of 'countries' and 'languages', and by the transmission of technology (both the results, i.e. the artefacts, and the knowledge of how to produce them) across groups, typically in the context of large ritual gatherings (see e.g. Jones and White 1988) and lengthy periods during which neophyte males received ritual and technical instruction from the host group.

The relevant historical transition that we are proposing, then, is from an 'isolated' to a 'linked' structure. We suggest that this took place, as a sort of

trans-continental chain-reaction, by the successive invitation of 'isolated' groups to become inducted in the ceremonial culture of the 'linked' Pama-Nyungan groups. For the ceremonial hosts this provided a source of spouses (making the ethnographically plausible assumption that the fee for induction was wives for the sons of the inductors) and the possibility of eventually dwelling in the wives' country. For the inducted, it provided access to 'powerful' new microlith technology (in both the materialistic and spiritual senses, stone-tipped spears being prized as much for their ritual power as for their material advantages (Jones and White 1988) and to a broader and more elaborate ceremonial culture than their parochial 'isolated' version. The suspicion of outsiders that would naturally have held groups back from induction would partly have been mollified by the sharing of what Beaton (1977) felicitously terms 'communion food' at these large inter-group gatherings.

Socio-economic organization of the two structures could have also differed along the lines suggested by Barnard and Woodburn (1988): 'immediate return' systems being more characteristic of the 'isolated' structures, and 'delayed return' systems more characteristic of the 'linked' structures. As proposed for Australian Aboriginal societies and more generally, control by middle-aged males over younger men and the disposal of women in marriage is accentuated in the development of 'delayed return' systems. Such control in ceremonial and marriage contexts would increase the potential for shift to the language of the controlling group which is expanding the network of 'linked' social structures.

The expansion of the Pama-Nyungan language would have involved both direct transmission (as Pama-Nyungan males married out into the inducted groups) and adoption as a prestige language associated with an expansive new culture, aided by the lengthy periods young inducted males would spend in ceremonial tutelage to Pama-Nyungan speakers.

Later western expansion

Probably coinciding with an improvement in climate following an arid phase, a group of Pama-Nyungan speakers (possibly Proto-Nyungic speakers) began to move out from riverine bases in Central Queensland about 3000–2000 BP to reoccupy the arid interior which had been largely abandoned, adapting technology and vocabulary as they went. This is the only phase which resembles Renfrew's (1987) 'initial colonization' and at least in its major expansionary period, does not require compromise with other groups or incorporation of elements from languages encountered. Later, however, some of these groups, having occupied the arid interior, began to interact with and move into better-watered peripheral areas, accomplishing language shift through the prestige of their social and ritual institutions, and incorporating linguistic elements from the residents (e.g. the northward movement of Ngumpin discussed in McConvell 1990, 1998).

Recent Western Desert expansion

Linguistic evidence clearly points to the Western Desert language having expanded in about the last millennium to cover a vast area of the most arid and inhospitable part of Australia. The phase of this group putting pressure on neighbours possibly leading to expansion back into the well-watered periphery had already begun probably before European contact but was both accelerated and radically altered by European occupation.

The model of successive spreads through a zone every couple of thousand years originating from homelands which successively move in the direction of the expansion, presented here and in Figure 7.2, bears a superficial resemblance to Nichols' (1993) theory of 'spread zones' applied, for instance, to language expansions in Asia. However, Nichols' theory provides little idea of why this particular pattern should exist, or what social mechanisms lie behind it.

The other model which can be related to ours is Sutton's (1990) concept of a 'pulsating heart', which has been conceived to explain the broad historical pattern of movement in Australia. Most emphasis is given, in the paper cited, to the phase of the pulsation in which interior peoples are driving down river systems towards the coast; however, the model clearly implies the other phase in which those on the interior periphery of the coastal zone move off into the interior under favourable conditions.

In these terms, Western Pama-Nyungan expansion represents an initial 'inward' phase followed by an 'outward' phase, and Western Desert expansion roughly the same, although the 'outward' phase is tangled with recent history. This raises the question of whether the initial eastern Pama-Nyungan expansion is also a two-phase pulse, with a group moving into the interior and expanding before engaging in interaction with coastal groups and eventually causing language shift to Pama-Nyungan among them. This scenario might yield distinctive linguistic outcomes from one in which people, for instance, moved directly along the coast. Further research is required on this.

Overall, however, this early eastern expansion would not necessarily involve movement of the same people or a large number of their immediate descendants throughout the area. It could equally be a process of chain expansion, whereby one group arrives with the new language and other cultural items, and after language shift has taken place in that area others move on to bring the new language to new areas, and so on. In this kind of process, biological markers of the original Pama-Nyungan population are soon attenuated as the expansion process continues, unlike in the case of expansion through sparsely populated areas postulated for the later western expansion.

In all three waves of Pama-Nyungan expansion a combination of material and ideological factors is involved. In the 'inward' phase of the pulse, material factors are dominant, in the sense that the opening up of new food resources in new areas and the technology to exploit them would be crucial. At the same time ideologies of alliance and interdependence between groups are

strengthened because ability to retreat into neighbours' areas would be crucial in the hazardous and unknown frontier environment (cf. Tonkinson 1988: 151–2).

In the 'outward' phase groups win over other groups that they contact to intermarriage and language shift by the ideological prestige of their ritual and social organization, as well as by new technology and food-processing techniques. One possible scenario is that initiates into the new ceremonial complexes would learn Pama-Nyungan as part of their religious socialization, and would subsequently use it with their offspring as a prestige language in their repertoire that would flag their knowledgeable position; this in turn would place them in a favourable position to negotiate marriage alliances between their offspring and other groups who have yet to adopt Pama-Nyungan ceremonial culture.

One of the elements of the ideological armoury of the spreading wave of Pama-Nyungan speakers may have been the patrimoiety system. As the patrimoieties came into contact with pre-existing matrimoiety and matriclan systems, the section system emerged as a combined system with different sets of terms used in North Queensland, Victoria, and the south of Western Australia. This system of classifying all people into four categories with fictive kin relations to each other is an important adjunct to the expansion of long-range alliances which we have suggested occurred in this period, because it enables people to establish kin relations with people, interact properly in ceremonies, and contract 'straight' marriages even when the partners are strangers.

Although our account is complex and involves several phases, each stage generates specific hypotheses for future research in linguistics, genetics and archaeology. For example, we predict that speakers of Pama-Nyungan languages should not be particularly homogeneous genetically, whereas speakers of Western Pama-Nyungan languages should; existing genetic research has tended to use the 'Pama-Nyungan' category far too loosely and more focused population comparisons are needed. In linguistics, substrate effects should be far clearer in Eastern Pama-Nyungan languages, due to the linguistic influence of substantial prior populations of non-Pama-Nyungan speakers. The different phases should also show quite different patterns of etymology. The 'inward' Western Pama-Nyungan expansion, dependent on new technology such as seed-grinding for exploiting food resources, should carry with it indigenous innovated words for these items and processes. The 'outward' Pama-Nyungan expansion should carry with it indigenous words for key social categories such as kinship, ceremony and wider social organization, but adopt substrate 'environment' words as loans.

CONCLUSION

We have shown that there is overwhelming evidence that the Pama-Nyungan languages are a valid genetic entity in linguistic terms. From a linguistic point

of view it looks very much like a group of languages whose proto-language was spoken some 4,000–5,000 years ago. It must have expanded from its homeland area and diversified in the period since then. A Mid-Holocene date for Pama-Nyungan expansion fits well with the archaeological record of expansion of new tool-types and changing economy and social organization in Australia at that time.

A simple opposition between 'hunter-gatherers' and 'agriculturalists' masks a whole range of complex organizational changes in human society, yet discussion of language expansion has hitherto been dominated by agriculture-driven models. A host of technological and social innovations are now known to have taken place in Australia over the last five millennia, and it is important not to underestimate the capacity some of these changes in hunter-gatherer societies have had to engender language expansion. In this chapter we have developed a multi-phase model for such language expansion among hunter-gatherers, which incorporates different social mechanisms for moving out of and into relatively highly populated areas. Although we have only looked at the Australian situation, we feel sure that patterns of language distribution among other groups of hunter-gatherers may reflect similar processes.

The biggest gap in our model is empirical verification from observations of language shift among contemporary hunter-gatherers. The comparative method can be used to show how languages are related to one another, but does not answer the question 'how did the expansion of language X occur?' To answer this question, the relevant disciplines are socio-linguistics and linguistic anthropology. Although our own field research in Aboriginal speech communities makes us regard the processes outlined in this chapter as intuitively plausible, there is a need for detailed documentation of the social mechanisms underlying the shift from one hunter-gatherer language to another in a range of hunter-gatherer societies, before we really understand a process likely to have occurred many times in the prehistory of such peoples.

ACKNOWLEDGEMENTS

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NOTES

1 Roger Blench (pers. comm.) suggests that the earliest phases of expansion of the Niger-Congo phylum preceded the arrival of agriculture and may require us to

postulate language expansion among hunter-gatherers. It would also be interesting to look at cases of language expansion within Khoisan, who show more ethnographic parallels to the Australian situation. At present, however, we know of no published account of hunter-gatherer language shift on a scale comparable to that we postulate for Late Holocene Australia.

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Part II

MIGRATION AND EXPANSION AND THEIR LINGUISTIC CORRELATES: EURASIAN CASE STUDIES

8 Ethnicity and language in prehistoric Northeast Asia

JUHA JANHUNEN

To the memory of Robert Austerlitz (1923–94)

NORTHEAST ASIA AS AN ETHNOHISTORICAL REGION

The geographical concept of Northeast Asia is here understood as the sum of the following five sub-regions: eastern Siberia, Mongolia, Manchuria, Korea and Japan (Chard 1974: xv). The maximum continental extension of the region is marked by the Yenisei in the west and the Yellow River in the south. Thus defined, Northeast Asia overlaps to a considerable extent, though not completely, with the geographical and cultural region of East Asia (Barnes 1993: 10–12).

Like any comparable part of the world, Northeast Asia provides material for a discussion of the methodological problems of ethnohistorical research. At the same time, there are several substantive issues of general interest that are specifically connected with the Northeast Asian region: the problem of Trans-Beringian ethnic connections, the rise of the 'barbarian' empires of East Asia, and the 'origin' of such major ethnic groups as the Koreans and Japanese, often considered ethnohistorically enigmatic.

SOURCES OF ETHNOHISTORICAL INFORMATION

Three different kinds of sources are available to ethnohistorical research: synchronic material on modern ethnic groups, historical records on earlier ethnic groups, and archaeological material on ancient human communities. Although the informative value of history and archaeology should not be underestimated in the case of Northeast Asia, the principal clue to ethnicity in prehistory is contained in the synchronic material. It is the synchronic situation that we hope to be able to see in retrospect when we search for ethnic connections between past and present.

The significance of synchronic material for ethnohistorical research lies in its inherent content of diachronic information. In principle, any ethnically diagnostic feature is a potential indicator of diachronic circumstances, such as ethnic contacts or movements. This means that ethnohistorical reconstructions are not necessarily dependent on the availability of archaeological or historical material. It is fully possible to approach the ethnic past of a given region on the basis of a diachronic interpretation of the available synchronic material only.

Archaeology and history are of immediate ethnohistorical value for restricted areas covered by a comprehensive and continuous documentation. There are few such areas in Northeast Asia, but a case in point is the Minusinsk Basin in southern Siberia, which exhibits a spectacular continuum of archaeological evolution and historical evidence extending over a period of four or five millennia, from Early Neolithic and Eneolithic (=Chalcolithic) societies (Tazmin-Afanas'evo-Okunevo) through a succession of Bronze and Iron Age cultures (Andronovo-Karasuk-Tagar-Tashtyk) up to locally produced medieval written documents (Runic Turkic) and further (Gryaznov 1969; Kyzlasov 1986).

PARAMETERS OF ETHNIC TAXONOMY

The synchronic identity of any natural ethnic group is composed of a multitude of parameters, which form three principal clusters corresponding to biological, cultural and linguistic differences. The relative importance of these clusters varies from case to case, but, as a rule, biological and cultural differences delimit larger and less coherent groups than can be established by linguistic criteria. The normal situation is that ethnic boundaries are congruent with linguistic barriers.

The distinctive role of language is even greater in the diachronic framework, for language involves a unique property which remains unchanged through time: genetic identity, as defined in the linguistic sense. While the biological and cultural identities of any human population, when followed backwards in time, fall apart into a confusion of heterogeneous strands, the genetic identity of a language forms, by definition, a single and unambiguously traceable lineage of descent. The only type of one-to-one correlation that can be established between present and past ethnic groups is based on the genetic taxonomy of languages.

THE LINGUISTIC CORPUS OF NORTHEAST ASIA

Depending on the criteria of drawing the distinction between language and dialect, there are some thirty to sixty languages presently or until recently spoken in Northeast Asia. Genetically they belong to ten uncontroversially established language families (Janhunen 1983): Ainuic (Ainu), Japonic (Japanese-

Ryukyuan), Koreanic (Korean), Amuric alias Nivkhic (Ghilyak), Tungusic (Manchu-Tungus), Mongolic, Turkic, Yeniseic (Ket-Kott), Yukaghiric (Yukaghir) and Kamchukotic alias Luorawetlanic (Chukchee-Kamchadal).

Of the ten families indigenous to Northeast Asia only two, Mongolic and Turkic, have members outside of the region, in Central and West Asia as well as Eastern Europe. Most other families in the region are concentrated on relatively small geographical areas of distribution: Ainuic and Japonic on the Japanese islands, Koreanic on the Korean peninsula, Yeniseic and Amuric in the Middle Yenisei and Lower Amur basins respectively, and Yukaghiric and Kamchukotic on the rivers and peninsulas of northeastern Siberia. The only language family spread widely over the whole region is Tungusic.

Additionally, there are four language families which marginally touch upon, or extend into, Northeast Asia: Eskaleutic (Eskimo-Aleut) in the northeast, Uralic (Finno-Ugric and Samoyedic) in the northwest, Indo-European in the southwest, and Sino-Tibetan (Sinitic) in the southeast. Thus, altogether we have knowledge of fourteen separate derivations of linguistic identity that can have been relevant to the formation of the present-day ethnic groups of Northeast Asia.

PROBLEMS OF DISTANT COMPARISON

Archaeologists often fail to recognize that there are two levels of genetic comparison: the level of agreed language phyla and the level of hypothetical macrophyla. For critical linguists, macrophyla exist only as unverified possibilities concerning distant (long-range) connections between the concretely established proto-languages (Austerlitz 1983). The hypothetical nature of such distant connections is clearly indicated by the fact that the affiliations suggested by different scholars for any single regular language family are often mutually exclusive (Austerlitz 1978).

Two of the most serious methodological problems about distant comparisons are, first, that they typically involve a random selection of non-binary relationships, a method also termed 'Omnicomparativismus' (Doerfer 1973), and second, that they contradict the fact that language families have a limited lifespan. At the lower end of the chronological continuum, the material foundation of any language family is being constantly erased at the same speed as evolution at the upper end causes genetic differentiation to take place. Most distant comparisons appear to refer to time-depths lying beyond the reach of the comparative method.

AREAL AND TYPOLOGICAL FACTORS

The most celebrated macrophylum in Eurasia is Nostratic, a huge entity supposed to extend from Uralic and Indo-European in the west to Koreanic

and Japonic in the east (Bomhard and Kerns 1994). The only families in Northeast Asia unanimously excluded from the Nostratic context are Yeniseic and Sino-Tibetan, which have been 'classified' as Sino-Caucasian (Starostin 1984). From the critical point of view, both Nostratic and Sino-Caucasian remain illusions based on unrealistic optimism and poor comparative work.

Considerably better arguments have been presented in favour of Altaic, a lower-level macrophylum which, in its extended form, comprises five genetic units: Turkic, Mongolic, Tungusic, Koreanic, and Japonic (Starostin 1991). However, even the proponents of Altaic disagree as to what languages should actually be included (Unger 1990), while the adversaries have shown that most of the linguistic parallels involved are borrowings (Doerfer 1985). All attempts to compare neighbouring entities on a binary basis, like Japonic and Koreanic (Whitman 1986) or Koreanic and Tungusic (D.S. Kim 1981), seem to have failed so far, corroborating the impression that there are fundamental linguistic errors in the Altaic conception (Janhunen and Kho 1982).

The languages termed Altaic also share a number of features with Uralic, nourishing recurrent speculations concerning a Ural-Altaic genetic affinity (Sinor 1988). Indeed, there is no reason to deny that the whole of northern Eurasia forms a single typological belt of agglutinative languages (Austerlitz 1970). The problem is that this parallelism can equally well be explained in terms of areal influences and typological convergences. The more information becomes available from the individual language families, the less need there is for a genetic explanation of the Altaic and Ural-Altaic types.

ETHNOHISTORICAL UNITY AND DIVERSITY

If all the hypotheses developed concerning distant relationships in Northeast Asia and elsewhere in the world were valid, there would be no other limit to ethnohistorical reconstructions than the point where the different genetic lineages based on linguistic relationships would ultimately unite in a single monogenetic source. The objective of ethnohistorical research would then be equal to describing the evolution from original unity to present diversity.

In the opposite framework, in which the linguistic problems of distant comparisons are properly recognized, we cannot hope to reach anything even close to an original unity. Instead, we face the considerably more challenging task of relating present diversity to original diversity. There is, indeed, no reason to assume that the ethnic map in the past would have been any less diversified than it is in the 1990s.

It is likely that for every type of cultural adaptation, there is an optimum value of ethnic and linguistic diversity per area unit (Austerlitz 1980). During periods of slow cultural evolution, as in the Palaeolithic, this value must have remained constant over long periods of time. Then, starting from the Neolithic, the potential for demographic expansion rapidly increased, leading

to decreasing ethnic and linguistic diversity. The resulting waves of linguistic expansion were accompanied by parallel waves of linguistic extinction. Large homogeneous areas, such as the Tungusic belt over most of Northeast Asia, may therefore be assumed to hide an earlier diversity far greater than the modern ethnic map suggests.

THE NATURALNESS OF SMALL FAMILIES

As it is, most of the regular language families in Northeast Asia are small and transparent, consisting of only a few relatively closely related languages. Some of the smallest families, such as Ainuic, Amuric and Japonic, have even been regarded as genetic isolates, but this is not exactly correct, since their 'dialects' are sufficiently diversified to form effective barriers to communication. The only language possibly corresponding to the definition of a true isolate in Northeast Asia is Korean (Koreanic).

The small genetic units of Northeast Asia stand in sharp contrast with the large and highly diversified language families distributed to the west and south, notably Indo-European and Sino-Tibetan. However, this apparent discrepancy need not imply that Northeast Asia should also ultimately turn out to have only one or two large language families. To the contrary, the ethnohistorical situation in Northeast Asia is probably more natural than that in most other parts of the Eurasian continent, where exceptionally favourable cultural and demographic circumstances have resulted in extraordinarily large and diversified language families.

THE LIMITS OF ETHNOHISTORICAL DEPTH

The general transparency of the language families in Northeast Asia has the unfortunate consequence that the corresponding proto-languages are chronologically located at a relatively shallow depth, probably ranging from a few hundred years for Ainuic and Amuric to a couple of millennia for entities like Turkic, Tungusic and Yeniseic. A longer history of differentiation is present only in the case of Kamchukotic, a circumstance that renders this particular family rather difficult to assess diachronically (Volodin 1969; Comrie 1981).

The shallow depth of most of the proto-languages in Northeast Asia also means that the corresponding prehistoric ethnic groups must be sought at relatively recent chronological levels. Some insights into the more ancient levels of ethnic formation can be caught by the methods of areal and diachronic linguistics, including internal reconstruction (Austerlitz 1981), but not too much should be expected of such additional sources of information. In much ethnohistorical research, the limits to knowledge are painfully close to the surface.

HOMELANDS, EXPANSIONS, INTRUSIONS

Whatever the chronological depth that can be reached, it is logical to assume that the proto-languages corresponding to the modern language families were once spoken by concrete ethnic groups, each occupying a limited area. The search for the original 'homelands' of language families is one of the principal procedures in all ethnohistorical research, though it may also be a source of frustrating disputes, as is exemplified by the case of Indo-European (Mallory 1989; see also Ch. 7, Volume I).

The relationship of the modern area of distribution of a language family to the original homeland can be explained in terms of either expansion or relocation. Relocations can have been triggered by expansions of neighbouring entities, and they often manifest themselves as intrusions into an originally alien environment, a development which, in turn, may have triggered new relocations and intrusions. Northeast Asia, as a rather marginal corner of the continent, has typically experienced a chain-reaction-like series of linguistic relocations, originally connected with ethnic expansions radiating from the continental centre.

There are two levels in the search for homelands: one geographical and the other archaeological. The geographical location of a proto-language is often possible by linguistic criteria alone (Dolgopolsky 1988), after which it remains the task of archaeologists to attempt an identification with a concrete prehistoric culture. In the following, the possible homelands of the ten language families indigenous to Northeast Asia are surveyed mainly from the geographical point of view, though occasional archaeological connections will also be pointed out.

Ainuic

In spite of persistent attempts to find affiliations with various other language families (Patrie 1982; Vovin 1993), the only thing reasonably certain about Ainuic is that it is a genetic unit which has been present on the Japanese Islands since Neolithic (Jomon) times. However, there is no need to assume that Ainuic has ever been the only language family in Japan. Much more probably, it is simply the only surviving example of an original diversity.

Although Ainuic has historically mainly been distributed on Hokkaido, with secondary extensions to Sakhalin and the Kuriles, the most likely ethnohistoric scenario would seem to be that Ainuic was once indigenous to northern Honshu, from where it was relocated further to the north under the expansive pressure of Japonic. This process of relocation must have resulted in the extinction of the 'original' languages of Hokkaido. The expansion of Ainuic on Hokkaido is closely reflected by archaeological material (Satsumon) directly anticipating the historically recorded Ainu culture (Ohyi 1984).

Ainuic is a rare example of a language family with a rather straightforward anthropological correlation (Levin 1963: 234–77). It cannot be ruled out that many of the physical features involved are 'archaic', in the sense that they

possibly continue a type which used to be more extensively spread over the islands and coasts of East Asia. However, although the specific Ainuoid physical complex forms a concrete part of the ethnic heritage of the Ainu, this connection between 'race' and language need not be primary in any sense. We shall simply never know, when, exactly, the historically recorded symbiosis of the Ainoid physical type and the Ainuic language arose.

Japonic

The problem concerning the 'origins' of the Japanese language has long stimulated comparativists in search for distant relationships (Miller 1980). Discussion continues, among other things, about the possibility of an 'Altaic' connection (Janhunen 1994b: Miller 1994; Vovin 1994). However, it is important to note that irrespective of what stand is taken with regard to the Altaic hypothesis, a continental derivation of Japonic seems inevitable in view of both linguistic and archaeological evidence.

Archaeologically it is well known that major ethnic invasions must have entered Japan from the continent in connection with the introduction of Bronze Age agricultural technology (Yayoi) and Iron Age social patterns (Kofun). Against the long continuity of the preceding cultural evolution (Jomon), these developments (*c.* 300 BC to AD 700) mark the first and last possible occasion when an expansive new language can have appeared on the Japanese islands.

Linguistically, the crucial evidence comes from the fact that a language closely reminiscent of Old Japanese was recorded, albeit fragmentarily, from parts of the Korean peninsula during the Three Kingdoms (Samgwug) period. Technically termed Old (Pseudo-)Koguryo, this language has often been regarded as a genetic link between Japanese and Korean (Miller 1979), but its unmistakable affinity with Japanese (B.H. Kim, 1981) allows it to be correctly classified as Para-Japonic, implying a branch of Japonic collateral to the Japonic idioms of Japan. The most likely explanation of the situation is that Japonic as a genetic unit was once spoken on the Korean Peninsula, from where it was only secondarily relocated to Japan.

Koreanic

The continental derivation of Japonic by no means excludes Koreanic as another major genetic unit equally 'indigenous' to the Korean Peninsula. Korea, like Japan, is a region large and diversified enough to have accommodated several culturally and linguistically distinct ethnic groups until relatively recent times (Nelson 1993). It would be naïve to think that the modern situation of cultural and linguistic homogeneity over the Korean Peninsula would have any particularly ancient roots. Historically the 'unification' of Korea under a single political entity (Shilla) was a prolonged process that took several centuries to complete (AD *c.* 500 to 1000).

It is the pre- and protohistorical coexistence of Koreanic and Japonic on the Korean peninsula that provides the best explanation of the profound typological parallelism, as is observed even between the modern standard

forms of Korean and Japanese. While the two languages may also have undergone later periods of interaction both across the Korean Strait and on the Japanese Islands, the basic underlying circumstance is that Korean has a Japonic substrate (Janhunen 1992: 10–13).

The question as to which of the two genetic units, Koreanic or Japonic, was more 'original' in Korea, seems impossible to answer for the time being. Ultimately, both language families may derive from the more continental parts of Northeast Asia, most immediately from southern Manchuria. As a good working hypothesis we may therefore include Japonic and Koreanic in a larger context that may be termed the Manchurian ethnohistorical region. While this hypothesis corresponds well to the linguistic facts, it remains to be seen to what extent it can be supported by archaeological data.

Amuric

Very much like Ainuic in the insular part of Northeast Asia, Amuric on the continent represents a residual entity pushed towards the north by linguistic expansions originating from central and southern Manchuria. Now represented by idioms separated by only a minimum of diachronic differentiation, Amuric may have earlier comprised a wider diversity of languages spoken in the Sungari-Amur basin. The linguistic material, though still insufficiently analysed, would seem to point to early contacts with Mongolic and Tungusic. The adjacency to Tungusic has historically resulted in the origination of contact populations (Ulcha and Neghidal), linguistically Tungusic but culturally transitional towards the speakers of Amuric.

The ultimate dead end in the northward relocation of Amuric was formed by the island of Sakhalin, where a contact was established between Amuric and Ainuic, with Tungusic intruding between the two even more recently. As all the three language families historically present on the island are thus relatively recent newcomers, the ethnolinguistic affiliations of the archaeologically documented earlier populations on Sakhalin (Kozyreva 1967) remain completely obscure and will probably never be recovered even by means of linguistic comparisons.

Tungusic

In view of the modern geographical distribution of the Tungusic languages and their genetic differences (Doerfer 1978), any archaeological identification for a Proto-Tungusic population should be sought in central Manchuria, where the greatest degree of linguistic differentiation within the family is concentrated. This is also the native region of the largest historically known Tungusic-speaking population (Jurchen-Manchu). Against this background, the whole Siberian extension of Tungusic must be viewed as the result of a secondary expansion, a circumstance also suggested by the conspicuous linguistic uniformity of Siberian Tungusic (Evenki-Even).

In the ethnic taxonomy of Chinese historical sources (Eberhard 1979), Tungusic clearly falls within the category of Sushen, though we must bear

in mind that none of the early categories of 'barbarians' needs have consisted of a single ethnolinguistic group. Similarly, there are indications that Mongolic and Turkic can be identified with the ethnic categories of Donghu and Xiongnu, respectively, but, again, with the reservation that it is not a question of one-to-one correspondences. Historical information is mainly concerned with political developments, while the accompanying processes of ethnic and linguistic evolution remain poorly documented.

Mongolic

The general ethnic evolution of the populations speaking languages of the Mongolic group can be relatively safely followed from Donghu through Xianbei to the historical Mongols. The problem is that the empire of Chinggis Khan, during the brief period of its political impact (AD *c.* 1200 to 1400), seems to have erased an earlier ethnic and linguistic diversity within Mongolic itself. A greater variety of Mongolic languages in the past is suggested by, for instance, some of the lexical elements borrowed from Mongolic into Manchurian Tungusic (Janhunen 1993).

While the ancestor of all the Mongolic languages surviving to the present day is, by definition, to be termed Proto-Mongolic, the extinct idioms collateral to it may be classified as Para-Mongolic. It is one of the most interesting challenges in the ethnohistoric reconstruction of Northeast Asia to identify ancient populations that may have involved speakers of Para-Mongolic. Good candidates are found, in particular, within the southern sphere of the Xianbei ethnocultural complex, from which some of the Early Manchurian dynasties, notably that of the Khitan (Liao), are known to have originated.

For the time being, we have no exact information as to what the linguistic distance between Para-Mongolic and Proto-Mongolic was, or what the degree of internal differentiation within Para-Mongolic may have been. It cannot be ruled out, however, that future research on the traces of linguistic interaction between Mongolic and Tungusic, as well as on the undeciphered script systems of northern China (Janhunen 1994a) will shed new light on these questions.

Turkic

The history of the triangle Mongolia–Manchuria–China is characterized by a cyclic movement of the centre of political power (Barfield 1989). The historical Mongols had their power base in Mongolia, but the earlier history of Mongolic and Para-Mongolic populations points to a more intimate association with Manchuria. There is, indeed, reason to assume that Mongolia is primarily the source region of the Turkic language family, while the Mongolic homeland was located further to the east, in western Manchuria. This scenario is strongly supported by linguistic material, when properly understood.

The principal clue to the correct ethnohistoric interpretation is contained in the huge corpus of lexical items shared by Turkic and Mongolic. In the framework of the Altaic hypothesis, these items are *a priori* regarded as evidence

of a genetic affinity between the two groups of languages, but in an areal framework can be analysed as loanwords from Turkic to Mongolic. This direction of borrowing is suggested, among other things, by the fact that some of the items exhibit a specific phonological innovation (rhotacism-lambdacism) only characteristic of a single branch of Turkic, technically termed Bulghar Turkic, as opposed to General Turkic.

The most likely historical correlation for the linguistic contacts between Bulghar Turkic and Mongolic is offered by the period of Xiongnu (Turkic) political dominance over Donghu-Xianbei (Mongolic). Towards the end of this period the Bulghar Turkic part of the Xiongnu conglomeration was forced to move westwards, where a single trace population still survives in the Volga region (Chuvash). The role of the dominant ethnic group in Mongolia was subsequently taken over by populations linguistically representing the General Turkic branch. The latter are then concretely identifiable with the founders of the historical Kök-Turk and Uighur empires.

Yeniseic

In the Trans-Eurasian Ural–Altaic continuum, Yeniseic forms a typological exception that can be due only to an ethnic intrusion from the south. Unfortunately, typology alone does not give any unambiguous hint concerning the actual source region of Yeniseic, for the non-Ural-Altaic features of Proto-Yeniseic (Starostin 1982) have parallels both in the east (tones) and in the west (grammatical gender). However, the fact that such features have been retained until the present day in an environment dominated by Uralic, Turkic, Tungusic and Mongolic suggests that the advent of the Proto-Yeniseic population in southern Siberia took place relatively recently.

From the linguistic point of view it is, therefore, impossible to identify Proto-Yeniseic with the Bronze Age cultures of the Minusinsk region, though such an identification (Karasuk) has been attempted on the basis of toponymic data (Chlenova 1969). However, Yeniseic must have been present in southern Siberia prior to the appearance of the historical Turks in northern Mongolia. This leaves us the Iron Age culture of the Xiongnu period (Tashtyk) as the single most likely archaeological identification for the intrusive Proto-Yeniseic population.

The assumption that the intrusion of Yeniseic into Siberia was connected with the ethnic movements of the Xiongnu period fits well into the context of the old hypothesis according to which the historical Kirghiz were descendants of a Yeniseic-speaking ancestral group. The Turkicization of the Kirghiz may well have begun already during the Xiongnu period, but a trace of the original Yeniseic language survived in the northern periphery of the new Siberian homeland.

Yukaghiric

Yukaghiric is the only genetic unit that still remains of the ethnolinguistic diversity that must have been present in central and northern Siberia prior

to the expansion of Tungusic, Mongolic and Turkic. Although historically divided into a number of separate tribal languages, Yukaghiric need never have been a widely diffused language family, nor need it ever have been the only language family in its region. More probably, it was native to a small network of nomadic communities which all the time coexisted with several other similar communities.

In its modern northerly location, Yukaghiric very probably represents an entity relocated from the more central parts of the continent. Historically, the Yukaghiric territory has been constantly shrinking from the south and west, pushing the language family to an increasingly marginal position in northeastern Siberia. This situation makes any archaeological identification of Proto-Yukaghiric virtually hopeless. Although information is rapidly increasing concerning the prehistoric evolution of the Siberian forest zone, there will never be binding evidence that could connect Proto-Yukaghiric with any particular archaeological culture.

Kamchukotic

Compared with Yukaghiric, Kamchukotic presents considerably more concrete problems. This is a language family frequently claimed to have Trans-Beringian connections with either Eskaleutic (Hamp 1976) or even 'Amerindian'. Indeed, if we accept the idea that the American native population descends from two major migratory layers, separated by a period of glaciation, then it would, in principle, be possible that a group (Kamchukotic) belonging to the earlier layer ('Amerindian') would have returned to Eurasia at the time when the second layer (Eskaleutic) was being formed. The problem is that there is no definitive linguistic or archaeological proof of this. There are, on the other hand, traces of early lexical contacts between Kamchukotic and Yukaghiric.

In the absence of any Trans-Beringian evidence, it appears suitable to view Kamchukotic as basically analogous to many other language families in Northeast Asia, in that it has a highly diversified southern branch (Kamchatic or Kamchadal) and a conspicuously uniform northern branch (Chukotic or Chukhee-Koryak). The northern branch is distributed over a vast region, suggesting a recent wave of rapid expansion, while the southern branch is concentrated in a geographically complex but territorially small area. It is this area in central Kamchatka, a region with a long tradition of well-documented local cultural development since Palaeolithic times (Dikov 1977–9) that would seem to be the likeliest candidate for a Kamchukotic homeland.

CONCLUSION

We have seen above that, even without the assumption of transcontinental genetic connections, it is possible to draw several important conclusions concerning the prehistory of the ethnic groups of Northeast Asia. The most important centre of expansion and, presumably, prehistoric cultural innovation, seems

to have been Manchuria, especially the region of the Sungari and Liao basins. This is from were the Proto-Tungusic speech community once started its northern expansion, ultimately covering most of Manchuria and Siberia. To the west of the Proto-Tungus, there must have been the homeland of the Proto-Mongols, while in the south, on the Korean Peninsula, two linguistic entities, Proto-Koreanic and Proto-Japonic, coexisted until the latter relocated to the Japanese Islands. To the west of the Proto-Mongols, in the physical territory of Mongolia, there was the homeland of the Proto-Turks, from which the historical Turkicization of much of Central and West Asia was initiated.

Compared with these expansive genetic units, there are entities which may be viewed as residual traces of an earlier diversity, pushed into increasingly marginal locations: Amuric, Ainuic, Yukaghiric, and Kamchukotic. Yeniseic, by contrast, is intrusive in its historical location, and it is likely to have been brought from Central Asia by a population anticipating the later nomadic expansions of the Turks and Mongols.

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9 *Cultural relationships in North-Central Eurasia*

LUDMILA KORYAKOVA

INTRODUCTION[1]

This chapter presents a model of cultural development in North-Central Eurasia in the second and first millennia BC. This period is of great importance for understanding long-term cultural patterns which conditioned ethnic and cultural processes in Eurasia. The archaeological material presented here is synthesized from the work of my colleagues who have studied the problems of the Bronze and Iron Ages of the Urals and Western Siberia (Kovalyeva 1991).

This region provides a unique opportunity to explore the principle of relationships between different cultures. Its geographical situation encouraged active contacts between peoples both in prehistoric and more recent times. The region is situated within several environmental and climatic zones (the South Ural and Kazakhstan dry steppe, the Western Siberia forest-steppe and forest) and this has left a mark upon the pattern of archaeological sites, which are characteristically diverse. The area under study, placed between the advanced south and the more slowly developing north, was part of a general prehistoric system. Basic social and economic changes there can be characterized in terms of an interdependence between regional changes and inter-regional dynamics. This explains the key importance of this region for understanding complex historical processes taking place in a broader area.

Nowadays, North-Central Eurasia is populated by different ethnic groups: speakers of Uralic, Altaic and Indo-European languages. In terms of physical anthropology they are represented by Caucasoids, Mongoloids and various intermediate types. The modern ethnic map was formed after the 'great folk movement', associated with the arrival of numerous Turkic-speaking peoples, which transformed the Eurasian ethnic and social landscape in the first millennium AD. Turkic languages replaced Iranian speech-forms, previously widely spread across a vast area of Eurasia (Ambroz 1981: 10).

Skeletal remains from cemeteries, dating to the time before the great folk movement, are of Europoid type. This relatively rapid linguistic and anthropological change in Western Asia is somewhat problematic, but it has been

argued that it was prefigured by earlier processes (Martynov and Alexeev 1986: 58–9).

It is commonly accepted that most of the inhabitants of the Eurasian steppe and forest-steppe in the second and first millennia BC spoke Indo-Iranian and Eastern Iranian languages (Mallory 1992; Parpola, Volume III). During this time they interacted with Finno-Ugric speakers who occupied the forest of Northern Eurasia (Smirnov 1966; Bongard-Levin and Grantovsky 1983; Kuz'mina 1994). The earliest cultures in this period were the Andronovo and Timber Grave lines of evolution, which were the foundation for the later cultural phenomenon known as the 'Scythian-Siberian cultural and historical unity' which was very significant in the first millennium BC. The forest-steppe was the contact zone where Ugrian and Iranian interaction took place.

Taking these hypotheses as a starting point, this chapter explores some of the basic contributing factors, means and results which are characteristic of such interaction and takes a closer look at the implications of conceptions of world systems to discuss not only regional, but also long-distance connections.

According to many present-day scholars, the Ugric epoch, when the ancestors of the Hungarians, Khanty and Mansi inhabited the same vicinity, lasted a little more than 1,000 years (second to the beginning of the first millennium BC). The transformation of environment and climate and a change in economic orientation had split the Ugric community into northern and southern branches by the early first millennium BC (Chernetsov 1953; Moguil'nikov 1972: 67–86; Fodor 1975; Veresh 1979: 179–88; Haydu 1985; Kosarev 1991). Southern Ugrians occupied the wooded steppe of Western Siberia, while pre-Permian groups of the Kama river basin were adjacent to Proto-Iranian speakers. Ugrian languages contain many words borrowed from Iranian (Abayev 1972; Parpola, Volume III) and there is some evidence that Ob-Ugrian mythology has a structure quite similar to that of the ancient Iranians (Bongard-Levin and Grantovsky 1983; Yashin 1990).

Recent archaeological research has revolutionized our ideas about Ural-Siberian cultures. Forest-steppe societies are now considered to have played a more significant role in Eurasian history than previously thought. Most of the evidence comes from burial mounds and from settlement sites, both open and fortified, illustrating the vibrant culture of the people who lived in the Urals in the second and first millennium BC.

GENERAL CONCEPTS

Some relatively stable cultural zones had formed in Eurasia by at least the second millennium BC. Russian archaeologists distinguish several cultures, associated with different environmental and climatic zones and they have proposed ethnic and linguistic affiliations for them. Although the degree of correlation between an archaeological culture and some concrete ethnic group

is arguable – indeed, many specialists deny even the possibility – Russian archaeology favours ethnogenetic studies. But over-enthusiasm for regional studies has resulted in a plethora of names for local periodizations and new cultures, without correspondingly fruitful theories of ethnogenesis.

Archaeological records *can* reflect some features of ethnicity, the most powerful marker of which is language. But progress in this field is limited by the difficulty of passing from scientific observations to a reconstruction of events. The identification of an archaeologically defined culture with an ethnolinguistic grouping should always be treated with caution. However, the use of models from ethnology and from theories of social evolution can allow us to present tentative hypotheses.

Any culture corresponding to an ethnic or social structure develops from the interaction between internal and external factors. The dynamics of cultural genesis can be revealed in the interrelation between tradition and innovation, which 'work' differently in different spheres. A culture passes through several stages: formation, progressivity, stabilization and transformation or disintegration. Stable stages are reflected better in archaeological material, usually represented by a number of similar sites within a given area. All the factors of social development act in balance, although this can be easily disturbed if even one basic component is changed for any reason (Koryakova 1991).

Periods of disintegration are not clearly seen archaeologically, but manifested by a discontinuity in a cultural tradition, a 'transitional period'. The destruction of social systems in prehistory was realized by a type of chain reaction; its start and finish did not necessarily coincide in time and space. Such disintegrations were accompanied by long or short folk movements lasting some four or five centuries (Chernyh 1989: 22). Cultures change their features, forcing us to look for their origins again and again. The creation of new social and ethnic units usually takes about two centuries, and therefore an archaeological culture which does not exceed two to three centuries, cannot be correlated with a particular ethnic group (Pavlenko 1989). When we deal with the ethnic interpretation of archaeological assemblages, we can only use macro-level concepts, applying them to linguistic families in relation to prehistoric populations (Shnirelman, Ch. 10, Volume I).

According to Russian ethnological theory, basic physical and geographical classifications should correlate with cultural/historical and ethnographic provinces. Such provinces provide a stable area, irrespective of the ethnic groups who inhabit them. In fact, territory is a component of the past and present, linking the accumulations of 'dead' culture in the lithosphere while functioning in the biosphere, representing 'living' culture. Analogous ecological conditions have an effect upon the formation of areas with similar cultural forms, best reflected in economic activity. Taking this into account we can use models of later cultures to investigate more ancient ones. Extending this reasoning leads to the principle of interconnection; a change within one culture leading to change in another culture – directly or indirectly, all at once or gradually.

The general hypothesis is that social evolution and devolution in North-Central Eurasia were cyclical. During the Bronze and Iron Ages there were several periods when some local population groups reached a high level of social development, creating complex economies based on metallurgy with cattle and horse pastoralism. The complexity of social organization was transmitted through active contacts, migrations, the exchange of ideas, people and technologies.

CULTURAL RELATIONSHIPS IN NORTH-CENTRAL EURASIA

From the beginning of the Bronze Age, several economic and cultural zones existed in Eurasia. They had in common relatively close material and spiritual traditions. A good example is the territory that encompasses Central Asia–Kazakhstan–Western Siberia–Southern Urals, where despite the changes in ethnic groups, the main relationships reproduced themselves periodically with different participants (Koryakova 1991).

Archaeological regions, provinces and localities are characterized by appropriate features: dwellings, cemeteries, pottery, etc. Steppe cultures seem to have changed rather rapidly, in contrast to the forest cultures where evolution was steadier and more low-key. The wooded steppes were a contact zone, but its cultures tended to gravitate towards the open steppe. The Urals were not an insurmountable barrier, but they did influence the directionality of interaction between the European and Asiatic cultural worlds.

The ancient metallurgy of the Urals, which emerged under the influence of the Caucasus and of the West was of great importance for North-Central Eurasia. Metallurgical centres developed in the Southern Urals in the third millennium BC, based on the Pit-Grave and Poltavkino cultures. Later other centres of bronze production appeared, of which the Abashevo centre was the most significant (Chernyh 1970: 123–5). Economic and social life became more active and the territory of the food production economy expanded. Urals metallurgy was transformed from the mid-second millennium BC. The local population began to explore trans-Uralic mineral deposits, which were richer due to the presence of copper ores. Eurasian metallurgy experienced a strong impulse from the Seyma-Turbino tradition, connected with a new technology, thin-walled melting. Independent centres formed in Kazakhstan and the Altai, which had rich tin deposits. Seyma-Turbino migrants moving from the East met indigenous groups in the Urals with a distinctive metallurgy and metalworking technology, based mainly upon arsenical bronze. Several archaeological sites in the Urals demonstrate a mixture of cultural and economic patterns. Tools and weapons were made mainly from tin bronze. The tin was imported from Kazakhstan and the Altai, stimulating exchange and intertribal connections across Eurasia. Metal from the Urals spread west up to the Dnieper river and competed successfully with Caucasian production.

During the second half of the second millennium BC certain classes of tools (axes, sickles, socketed axes, adzes, chisels, knives, knife-daggers, pins, spears and arrowheads) were in common use all over the Eurasian steppe and forest-steppe and also penetrated the forest zone.

At the beginning of the second millennium BC, a large population movement occurred in the Eurasian steppe, expressed by the diffusion of cattle pastoralism, metallurgy and chariots. In the South Urals, the Poltavkino culture peoples (a late variant of the Pit Grave line of cultural evolution) moved in from the west, and an Abashevo population followed them. This period is also marked by Arcaim-Sintashta assemblages, discovered in the South Trans-Urals (Zdanivich 1989; Guening *et al.* 1992). They are represented by circular fortresses with a regular plan and distinctive architecture, interpreted as proto-cities, concentrated in a relatively limited territory, which had rich natural resources, including copper ore necessary for bronze metallurgy. The fortresses are associated with burial mounds containing the graves of charioteers (the chariots had spoked wheels) and lavish animal sacrifices. Kuz'mina (1994) thinks that the first inventors of battle chariots with horse-harnesses of disc-like cheek-pieces were the inhabitants of the Volga-Ural region. Originally they were of Catacomb-grave and Manyroller ceramic cultures, and influenced the foundation of both the Potapovka and Sintashta-Petrovka cultures. The greatest concentration of this type of horse-harness is in the territory between the Don river and the Trans-Urals. Disc-like cheek-pieces came to Kazakhstan, the Danube basin and, ultimately, to Greece from the South Russian steppe (Kuz'mina 1994: 171–87).

The presence of Sintashta-Arcaim tribes in the Urals is dated to between the twentieth and sixteenth centuries BC, based on a calibrated radiocarbon date for the Krivoe Ozero site (Anthony 1993). Kuz'mina (1994: 377) accepts a seventeenth-century-BC start date for the Petrovka-Sintashta assemblages. The Sintashta-Arcaim tribes brought new kinds of economic practice and social structure, new burial forms and architecture, and wheeled chariot technology, previously unknown in the region. The social organization of this population was fairly advanced and most probably it had a chieftain-based hierarchy and a developed cosmology. The discovery of this archaeological complex has revived the so-called 'Aryan problem'. The Arcaim-Sintashta sites *may* reflect traces of the gradual southwards Aryan migration towards India (Guening *et al.* 1992: 6–10).

The first half of the second millennium BC can be described as a period of active ethnic, economic and social integration on the basis of advanced cultures. Two basic Eurasian cultural confederations were formed: Andronovo in the East and Srubnya (Timber grave) in the West. The archaeological sites of the South Urals and Trans-Urals within the steppe and forest-steppe, consisting of long-settled villages and cemeteries, provide evidence for the variety of interactions between these super-ethnic areas. The vivid and extraordinary features of Sintashta-Arcaim culture later disappeared, but surprisingly they continued into the Iron Age in the forest-steppe, with circular fortresses and burial mounds.

By 1400–1200 BC, first the Alacul then the Fedorovo cultures pushed into the forest-steppe, towards the northern taiga, evidenced by numerous settlement sites with pottery showing distinctive ornamentation (Potyemkina 1985; Koryakova *et al.* 1991). The indigenous population took up pastoralism and began to use more tin-bronze. The populations were transformed from 'stockbreeders, hunters and fishermen' to 'stockbreeders, metallurgists and hunters'. The consequence was a population redistribution and transformation within the vast area east of the Urals. Thus, around the mid-second millennium BC, various groups initiated a shift of the steppe population to the North.

The result of this interaction was the Andronovo cultural province, in the territory from the Urals to the Yenisei river. A similar process has been traced archaeologically in the European forest-steppe. According to palaeogeographers, climatic conditions in the mid-second millennium BC promoted a pastoral economy with minor cultivation (Kosarev 1981, 1991). The Ugrian population was related to this culture, with an Andronovo ornamental tradition, characterized by elaborate comb decoration on flat-bottomed pottery, coming from large timber-built houses (Kosarev 1991). Cemeteries are unknown, except in the most recent variants (Mejovo-Irmen').

To support a Proto-Iranian (or Indo-Iranian) linguistic affiliation for the Alakul and Fedorovo cultures (or branches of the Andronovo cultural confederation) we must suppose that the areal extent of these languages increased and partly displaced Ugric languages. This is a provisional hypothesis, as there was probably a 'linguistic pendulum' in the intermediate territory. Later, the reverse situation occurred, a shift of the forest culture and population to the south, and by the mid-first millennium AD the new southern wave would reach the forest-steppe. The forest-steppe environment was responsible for a mechanism of cultural genesis which was flexible and allowed incorporation of innovations without prejudice to local cultural originality. This mechanism acted in both the Bronze and Iron Ages. The fact that no early Andronovo culture cemeteries are known from this region suggests that the local population retained its own mortuary practice and, consequently, its own ideology. It adopted the pastoral economy and some elements of the new material culture.

The end of the second millennium BC is marked by a general transformation. Instead of the former cultural standardization, a diversification of traditions can be traced first in the steppe, then in the forest-steppe. Changes began in the south, were enlarged upon in the north and created something like a 'wave effect', dying out in the forest.

Urals bronze metallurgy flourished in the second millennium BC and formed the basis for high cultural development in Eurasia (Chernyh and Kuzminyh 1989). Over time, it gradually lost its productivity, but a new development has been connected chronologically with the transition to the Iron Age. At the end of the Bronze Age (the ninth to eighth centuries BC) new metallurgical centres appeared in the Urals (Ananino in the west, Itcul in the east). They were closely connected, having a common basis continuing certain

traditions, but their spheres of influence differed: the first had a Scythian cultural orientation, the second a Sakas zone orientation. In the forest-steppe and northern steppe between the Urals and the Ob River the Bronze Age lasted until the sixth to fifth centuries BC. It was characterized by the development of a non-ferrous metal industry, although iron had already become common in Europe by the eighth century BC. The forest-steppe inhabitants of the European region mastered iron technology rapidly and effectively and became suppliers of iron to the Scythians. The seventh to sixth centuries BC were the transitional period to the Iron Age for much of Siberia. The fifth to third centuries BC were the time of the formation of a true ferrous metal industry in the forest-steppe. From the third century BC iron tools and weapons were made in a greater quantities, but the use of iron became general only at the end of the first millennium BC.

The irregularity of technological advance is reflected in the varied cultures of Eurasia in this period. But there is a pattern; the transition to the new technology was accompanied by the disappearance of the old ones and by the formation of new cultures, or at least, of new cultural and social forms. The introduction of metallurgy can be regarded as one of the most significant stimuli for interaction between different areas. The bronze industry, which had to be close to raw material deposits, promoted centralized cultural development. In the second millennium BC, the south was more advanced while the north remained almost neolithic. Later, as iron was introduced, the area of advanced development became much wider and the differences between areas of Eurasia diminished. By the mid-first millennium BC almost all Eurasian societies participated in a more general historical process, leading to rapid cultural development and high social mobility.

At the beginning of the first millennium BC, the steppe population had changed its economic orientation to nomadism. It already had long experience of pastoralism, horse breeding and efficient means of transport. Horseback riding supposedly originated at about 1200 BC (Kuz'mina 1994: 192–3). Since that time the main direction of long-distance migration was from east to west and regional communication networks began to function more effectively. The caravan trade probably originated in the first millennium BC: camel bones are frequently found in settlement sites in many regions, including Western Siberia (Koryakova 1988; Matveeva 1993)

The borders of the new State societies approached the barbarian periphery throughout Eurasia. The Eurasian world had already been divided into several spheres of influence. The Eastern European cultures gravitated to the Classical centres, the Asiatic ones towards China. Western Siberia and the Kazakhstan steppe were part of a cultural and economic system that included the Central Asian states. Archaeological material testifies to Greek and Roman imports reaching the Volga river in the east, and sometimes penetrating as far as the Urals. Chinese goods were frequent on the Irtysh river and in the Altai region, especially after the fifth and fourth centuries BC. Sites in Western Siberia usually yield goods from Central Asia, although numerous glass beads

Table 9.1 Economic specializations in North-Central Eurasian cultural zones

Environment	*Archaeological culture*	*Period*	*Subsistence strategy*	*Possible linguistic affiliation*
Steppe	Ardgan-Chernogorovo stage of Scythian-Sakas cultures	Iron Age	nomadism	Iranian
Forest-steppe	Mejovo-Irmen	Final Bronze Age	pastoralism	South Ugric
Forest	Lozva-Atlym	Late Bronze Age	hunting, fishing	North Ugric or pre-Samoyed (Kosarev 1991)

found there come from the Mediterranean, Asia Minor, the Middle East and India (Matveeva 1993: 159).

On the eve of the transition to the Iron Age, North-Central Eurasia displayed the cultural zones, differing according to economic specialization, shown in Table 9.1.

Between the ninth and seventh centuries BC there was a migration of the forest population towards the forest-steppe. This shift followed higher precipitation in the humid zone and the diffusion of forest vegetation to the south. This forest-steppe culture spread south and many fortified settlements appeared (Borzunov 1992).

An important factor of interrelationship in the Iron Age was the division of labour, which promoted the more or less stable functioning of economic and social systems within the area under study. The new metallurgical centres, situated on both slopes of the Urals, were oriented to the nomads' needs by supplying them with weaponry. These were metal-producing cultures with a particular specialization. The division of labour between societies may be connected with different ecologies and contribute to mutual exchange. Military and political factors, conditioned by nomadic activity and expansion, reinforced the tendency to internal integration.

In the fifth century BC there was a turning point (many substantial events happened around this date) for North-Central Eurasia and a new cultural system appeared in the Western Siberian forest-steppe. Numerous sites display a hierarchy of settlements and cemeteries, known as the Sargat culture or cultural community (fifth century BC to fifth century AD), a clear example of relationships between different economic systems and social structures. This culture formed through interaction between the nomads (Sakas and then Sarmatians) and the local population. The model here would be 'cattle breeders, hunters and nomads' (Koryakova 1988, 1991). The situation in the mid-second millennium BC was repeated, and the pendulum swung back to Iranian influence (if indeed the Sakas and Sarmatian tribes spoke Iranian languages). A similar pattern later underlay Siberian Tartar state formation in the fourteenth and fifteenth centuries AD (Okladnikov 1968: 360–6).

The Sargat cultural confederation developed as a multi-component social and ethnic unit, although the main component was the Southern Ugrians, since that local tradition persisted. Despite the impact of nomads, who traditionally had summer pastures in the southern forest-steppe, there was no radical change of population. The nomads passed on their knowledge of horse breeding and training and the local population adopted the nomadic systems of funerary ritual along with features of social stratification. Iranian mythology was diffused to the Ugrians in the second half of the first millennium BC – at the period of the closest relations between Iranian nomads and Southern Ugrians. Later, elements of Iranian cosmology would be mirrored in the mythology of the Ob-Ugrians. The Sargat confederation also included different groups of Samoyed tribes (Kulay Culture) and probably some Proto-Turks (Huns). The core of the future Hun army was composed of Ugrians – descendants of the Sargat culture – and by more militant Turks. The archaeological material, including fortresses and cemeteries with a complex system of funerary ritual, suggests that Sargat society was organized into chiefdoms. It persisted until the middle of the first millennium AD but was overthrown by internal (ecological and economic collapse, demographic tension, social contradictions) and external (complex relations with the Kangu kingdom and with the Huns) factors. This culture completed the Andronovo line of evolution in North-Central Eurasia.

CONCLUSION

Over two millennia in North-Central Eurasia several cycles of cultural development occurred. Active intertribal connections, conditioned by ecology, economy and demography influenced various cultures differently. On the one hand, they promoted rapid technological diffusion and the spread of a food-producing economy to a broader region. On the other, the periodical impact of new populations interrupted the continuity of ethnocultural evolution. Although several stages of alternating cultural integration and disintegration can be distinguished, this did not imply a total replacement of population. In the earlier period the pattern of interaction was between people speaking Finno-Ugric and Iranian languages, followed in the Middle Ages by Finno-Ugric and Turkic speakers.

NOTE

1 This chapter was sketched in English and extensively rewritten by Roger Blench and Matthew Spriggs, who take responsibility for any distortions of meaning that have occurred in the process.

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10 *The Eurasian spread zone and the Indo-European dispersal*

JOHANNA NICHOLS

GEOGRAPHICAL TERMINOLOGY

- *Central Asia* refers to the entire dry interior of Eurasia from the Caspian to Inner Mongolia and from the southern reaches of the Siberian forest to the Hindu Kush, the Pamirs, northern Tibet, and northern China.
- *Western Central Asia* corresponds to former Soviet Central Asia (Western Turkestan in traditional terminology): from the Caspian to the Altai and Tien Shan mountains, the modern countries of Kazakhstan, Kirgizstan, Uzbekistan, Tadjikistan, and Turkmenia.
- *Eastern Central Asia* is modern Xinjiang (earlier Chinese Turkestan) and Mongolia, including Inner Mongolia.
- *Anatolia* refers to the area of modern Turkey and some of northern Mesopotamia, i.e. roughly from the Mediterranean to the south Caucasus.
- The *Danube Plain* is the grassland area of Central Europe, roughly present-day Hungary and parts of Slovakia and Romania.
- *Bactria-Sogdiana* is the southeastern rim of western Central Asia: southeastern Turkmenistan, southern Uzbekistan, and at least part of Tadjikistan, comprising the piedmont, intermontane valleys, and adjacent lowlands.
- The steppe is conventionally divided into three segments: the *Black Sea* (or *Pontic*) *steppe* running from eastern Europe to the Ural Mountains; the *Kazakh steppe* from there to the Altai range; and the *Mongolian steppe* to the east. The Black Sea and Kazakh steppes are a single continuous grassland, narrowed but not cut off by the Urals; the Mongolian steppe is separated from these by mountains and desert (the desert passable to riders and wagons).

INTRODUCTION

The Central Eurasian grasslands which form the early range of Indo-European are a good example of a spread zone – a region where a single language

family spreads out widely, genetic diversity of languages is low, and one language family replaces another over most of the area every few millennia. Language succession of this type has been illustrated in the region more than once during historical times. Throughout much of the first millennium BC the Eurasian steppe and the deserts of Central Asia were ruled by speakers of Iranian languages, whose best-known representatives on the steppe were the Scythians. In the early centuries AD the Iranian languages were replaced by languages of the Turkic family which spread from the vicinity of Mongolia to cover the steppe and desert, reducing the Iranian languages to a fringe at the edge of the grasslands. In the early Middle Ages the Turkic languages began to be replaced by Mongolian languages which spread from Mongolia. This latest spread was never completed; in the late Middle Ages the tide turned and, for the first time since the early Neolithic and quite possibly much earlier, the general trend of linguistic spread became eastward and Russia began its rapid colonization of the steppe, Central Asia, and Siberia.

Inquiry into the location of the Indo-European homeland needs to be conducted in full awareness of the fact and the mechanism of language spreading over the Central Eurasian grassland, for the homeland, no matter where one places it, is either in or near the grasslands. Language spread is a matter of linguistic geography, and the purpose of this chapter is to propose a theoretical apparatus for describing linguistic spreads in linguistic and linguistic-geographical terms. In the absence of any such apparatus, the large literature, both linguistic and archaeological, on the Indo-European (IE) homeland has tended, with varying degrees of explicitness, to see the IE spread as a demographic phenomenon, invoking migrations or expansions to explain it. But such explanations leave several aspects of the IE dispersal unaccounted for. Linguistic features of the spread that require explanation include the following seven.

1 The age of IE. Several kinds of linguistic evidence reviewed below converge on a date around 3700 BC as the time of the IE linguistic breakup and the late fourth millennium as the earliest date of loss of linguistic community.
2 The full geography of the IE spread. The early location of the Anatolian daughter branch in Asia Minor; the Indo-Europeanization of Europe; the early spread to India; and the location of Tocharian far to the east of historically attested IE territory.
3 The structure of the IE family tree. While linguistic families of its age range generally show from one to three initial branches, PIE (Proto-Indo-European) split into a dozen or more branches, not strictly simultaneously but nearly so. This extensive early branching is unusual and calls for explanation.
4 The *centum–satem* division. This distinction splits the IE daughter branches into two groups, and does so at the PIE level, yet without creating a genealogical split, i.e. a node in the family tree. This fact, and the geography of the *centum–satem* isogloss, must be accounted for.

5 The distribution of typological diversity within IE. Typological divergence from the inherited morphosyntactic pattern is especially pronounced in some parts of the IE world: insular Celtic and especially Irish (verb-initial order, head-marked prepositional phrases, configurational noun phrase with article); the Balkan languages, especially Albanian and modern Greek (verb-object order; second-position or ad-verbal clitics marking major clause functions, with a trend toward clitic doubling; configurational noun phrase with article); Anatolian (ergativity; second-position clitics with clitic doubling); and Indo-Iranian and especially Indic (ergativity; heavy reliance on causativization in the verbal lexicon). A homeland theory must account for why typological divergence is so pronounced precisely in these areas.
6 The PIE culture as we reconstruct it on the linguistic evidence, including the textual evidence for a mythology and a poetic tradition.
7 The histories and geographies of central Eurasian language families other than IE.

These are important issues in the linguistic geography and linguistic identity of IE, and a posited homeland will win the acceptance of linguists only to the extent that it is consistent with these aspects of reality. All homelands posited by archaeologists attempt to account for (2), the geography of the IE spread: for example Anthony (1991, 1995), Mallory (1989) and Gimbutas (1956, 1977, 1991) place the homeland on the western steppe at around 6000 BP, while Renfrew (1987), Zvelebil and Zvelebil (1988, 1990) and Sherratt and Sherratt (1988) place it in the vicinity of Mesopotamia at up to 10,000 BP. The tradition represented by Anthony, Mallory, and Gimbutas additionally attempts to account for the age of IE (1), the structure of the family tree (3), and the culture (6). Contributions from human genetics (e.g. Ammerman and Cavalli-Sforza 1984; Sokal *et al.* 1992; Cavalli-Sforza *et al.* 1994; Piazza *et al.* 1995) are concerned chiefly with geography (2) and little else. Proposals that trace the PIE dispersal to the dispersal of agriculture (Renfrew 1987 and others) attempt to account not for the full picture of IE linguistic geography but only for the single fact of the wide range of IE; they are at odds with the known age (1), the family tree structure (3), and the cultural reconstruction (6). Linguistic proposals are generally concerned with chronology (1), geography (2), family tree structure (3), and culture (6). Many linguistic proposals of homelands offer accounts of such things as the *centum–satem* split (4) and the family tree structure (3), but none gives a solution that is demanded, rather than simply borne, by these facts. No proposal accounts for the diachronic linguistic geography of Eurasia (7). This chapter aims to account for all seven points, and furthermore to give a solution that is demanded by the facts.

MODELLING HOMELANDS IN LINGUISTIC GEOGRAPHY

This section proposes some abstract principles governing the geographic distribution of languages in spread zones, by which the IE dispersal can be reduced to general principles. The IE spread and family tree structure are distinctive in some respects among well-studied families of comparable depth, but it is the specific geography and not the general fact of spreading that can be shown to be distinctive; the history of IE is absolutely ordinary for its homeland, differing at most in degree and detail from the histories of other families from the same general homeland area.

Before proposing a model for the IE homeland and spread it will be useful to identify the assumptions about language spread that have guided previous research. The following assumptions seem to me to underlie all contributions to the IE homeland question:

1. PIE was spoken in some locale and spread out widely only after its break-up. Homeland studies seek to establish the approximate boundaries of PIE, and those boundaries were very different before and after the break-up of linguistic unity.
2. As major economic innovations (such as agriculture, stockbreeding, horseback riding, or wheeled transport) spread, languages spread with them. These are demographic spreads, and the language is carried by the expanding or migrating group. (A theoretical framework for associating the spread of agriculture with a major demographic spread is presented in Ammerman and Cavalli-Sforza (1984). A theoretical framework for migration is given by Anthony (1990)).
3. The spread of IE to cover most of Western Eurasia is striking, even a singularity, and a unique cultural and/or economic development (such as the rise of agriculture) must be posited to account for it.
4. The location of the Anatolian branch of IE (Hittite and its sisters) is a problem, or at least a puzzle, for IE homeland studies. The Anatolian languages are attested very early in Asia Minor, removed from Europe and far from the steppe; Gamkrelidze and Ivanov [1984] (1994), Diakonoff (1985), Renfrew (1987) and Sherratt and Sherratt (1988) offer as a strength the ability of their proposed homelands to account for the location of Hittite with minimal migration. Alternatively or additionally, the location of Tocharian – attested in the early centuries AD well to the east of most IE territory in present-day Xinkiang (Chinese Turkestan) – is a problem or a puzzle (e.g. Sherratt and Sherratt 1988: 589 n. 4). Accounting for the locations of both Hittite and Tocharian is usually presented, at least rhetorically, as a major problem.

Not all works on the topic make use of the principle of centre of gravity or least moves (Sapir [1916] 1949; Dyen 1956: the second term is Dyen's), whereby the homeland is to be sought in the vicinity of the deepest branching of the linguistic family tree. But those that do, make explicit use of a fifth assumption:

5 The IE homeland is to be sought where the greatest number of primary IE daughter branches come together on the map.

In fact, however, for a family arising in a spread zone none of these assumptions applies well. In what follows it will be shown that the IE spread, far from being a singularity, is an instantiation of a standing pattern that probably began before PIE and certainly continued after it. In a spread zone, the homeland and the spread are not two distinct things but aspects of a single phenomenon. In the Eurasian spread zone, languages have tended to move against the direction of major economic innovations such as agriculture and stockbreeding, and linguistic spreads are not mainly demographic spreads but involve a substantial amount of language shift, so much so that they are best modelled as language shift pure and simple. The location of the homeland proposed here predicts almost automatically the locations of Anatolian and Tocharian. The entry to a spread zone is a classic bottleneck, and genetic diversity of languages, far from marking the point of origin, accumulates at the far periphery of the spread.

This chapter is programmatic, intended only as a development and application of linguistic principles in order to identify a homeland on strictly linguistic grounds, completely independent of archaeological evidence. The proposed homeland is a geographical estimate, not a precise specification, and like any estimate it involves a certain amount of abstraction.

The abstraction lies in the following. The imprecise notion of location will be broken down into three distinct concepts, which I will call *locus*, *range*, and *trajectory*. (Henceforth I will use *location* and *homeland* only as general terms subsuming these more precise notions.) These concepts correspond approximately to the standard geographical notions of centre, distance, and direction, and are offered as technical terms for specifically linguistic, or sometimes more generally cultural, distributions. The locus is a centre of sorts, analogous to the centre of a dialect-geographical area; it can be localized (though not precisely) in space and time; it is a point-like construct in terms of which range, trajectory, isoglosses, and other aspects of distribution can be described. (The PIE locus is described in more detail in Nichols, ch. 8, Volume I.) The range is the spatial extent of a language's distribution, i.e. the distance from the locus to the periphery. In homeland studies it is impossible to specify the range precisely, but the important issue is to decide whether or not there is a substantial difference between the range and the locus. For IE, as will be shown, there is; the locus is fairly well defined, but the range is extensive.

The trajectory is the overall directionality of linguistic movements and spreads. That directionality is an overall one, from which there can be local or temporary departures; indeed, a trajectory should be posited only when it is on a scale of centuries or millennia and hundreds to thousands of miles. Two kinds of trajectories will be dealt with here. A loanword trajectory is a diffusion channel along which loan vocabulary spreads from a prestigious

culture to its neighbours and their neighbours. A language trajectory is a general directionality of the spread of whole languages or language families. Again, a single spread does not create a language trajectory; a good case for a trajectory can be made only when different languages or language families can be shown to have spread along the same general route at different times. A trajectory, in short, is a long-term or standing pattern of spreads. A language trajectory is an integral part of the reconstruction of a PIE homeland and explains the historically attested locations of the daughter languages.

An estimate of location, then, consists of estimates of locus, range, and trajectory. Below it will be shown that range can be predicted from locus and trajectory, and that what is usually seen as a location problem for PIE is more specifically a range problem.

Figures 10.1 and 10.2 illustrate the notions of locus, range, and trajectory for northern Eurasia. Figure 10.1 shows the range and locus for two European families that underwent large spreads, Celtic and Slavic, and one that did not, the compactly distributed Nakh-Daghestanian (Northeast Caucasian). The Celtic and Slavic spreads took place at different times: Celtic spread in the mid-first millennium BC, and Slavic in the fifth or sixth century AD. The locus for Celtic is provisionally placed in the area of the La Tène culture from which the Celts are generally regarded as having spread. The locus for Slavic is placed in the vicinity of the western Danube plain, which is the evident centre of cultural influence from which the spread of Slavic speech emanated, and an early centre of Slavic political power. (The Slavic spread will be discussed again on pp. 242ff.) Also shown is the area farther to the east, around the middle Dnieper and extending at least to the Carpathians, that is generally taken by archaeologists and linguists to be the territory of the earliest identifiably Slavic culture and the Proto-Slavic linguistic homeland. In the terms used here, this middle Dnieper homeland may be the origin from which Slavic expanded to Central Europe before beginning its rapid spread from that locus; or perhaps the locus included both the Danube plain and the middle Dnieper centres and was therefore larger than shown Figure 10.1.

For both Celtic and Slavic the range is much larger than the locus. This is not true for Nakh-Daghestanian, also shown on Figure 10.1, which has spread very little beyond its locus. The small Nakh-Daghestanian spread began several millennia ago, after the domestication of cattle, sheep and goats but before horse domestication reached the Caucasus, and possibly before the Bronze Age (to judge from the reconstructible lexicon, which includes a developed livestock terminology but no word for 'horse', and which also lacks terms for metals such as bronze). This would point to a date around 6000 BP, which is consistent with the glottochronological results for the Daghestanian branch reported by Gigineišvili (1977): over 4,000 years for the Daghestanian branch alone, with no correction for adjacency and close areal interaction among the languages; Alekseev (1985: 23) reports comparable percentages of cognate retention but does not compute glottochronological ages. In Figure 10.1 the Nakh-Daghestanian locus includes the earliest agricultural sites in the northern

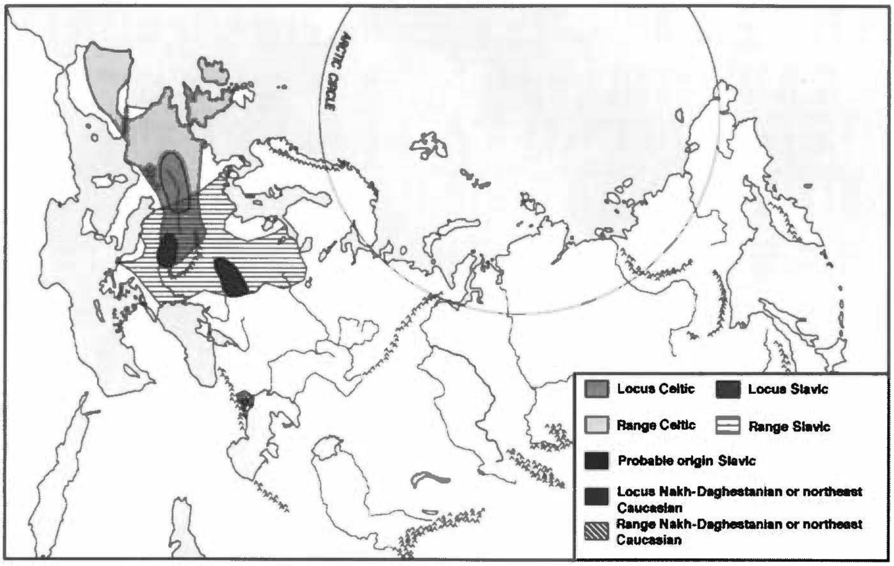

Figure 10.1 Example of locus and range of particular language spreads

Daghestanian mountains, whose culture arguably shows continuity down to the present day (for the Eneolithic and Bronze Age in the northeast Caucasus see Gadžiev 1991). Since languages in mountains tend to spread uphill, and since the lowlands around the eastern Caucasus are part of the central Eurasian spread zone and therefore one language or language family tends to occupy a large swath of the plains (presently, Turkic Kumyk north of the Caucasus and Turkic Azeri to the south), I assume Proto-Nakh-Daghestanian began its mountain history not with a point-like locus but with a band-like locus extending across the eastern foothills to approximately the altitude of the early agricultural sites. There has been some spread, both uphill and into the plains, but it has not greatly expanded the territory beyond the locus. Despite its small territory, the Nakh-Daghestanian family is older and has more and deeper branches than Celtic or Slavic.

Figure 10.2 illustrates the notion of trajectory with two kinds of trajectories in prehistoric and early historic southwestern Eurasia. The solid arrows show the general directionality of the spread of the Neolithic revolution, and especially stockbreeding, from the centre of innovation in eastern Anatolia and northern Mesopotamia. As the cultural influence of ancient Mesopotamia continued to spread along the same trajectories, they are taken here as loanword trajectories. The three loanword trajectories shown in Figure 10.2 were not equal in chronology or prominence: the western route, through the Balkans, was earliest and probably strongest; at least it is generally credited with playing the major role in bringing agriculture and stockbreeding to Europe.

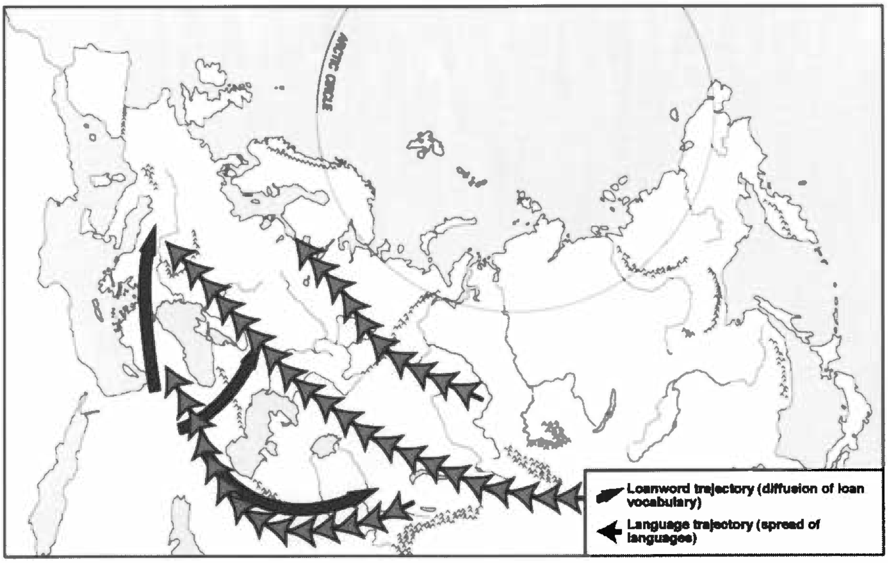

Figure 10.2 Central European spread zone

Three language trajectories are shown in Figure 10.2, and these are contemporaneous but not equal in prominence or carrying power. The central one, henceforth the *steppe trajectory*, shows the east-to-west spread of languages across the Eurasian steppe, and is based on four spreads: that of IE to Europe in the Bronze Age, that of Iranian to (and occasionally into) Central Europe during the Iron Age, that of Turkic in the early centuries of this era, and that of Mongolian beginning in the Middle Ages. To the north of it is the almost equally extensive *forest trajectory* through the northern forests. This is the route followed by the Uralic language family in its spread from the central Urals (*c.* the fourth or fifth millennium) as far west as Norway and Estonia (by perhaps the first millennium BC). To the south of the Caspian and Black Seas runs the *southern* or *desert* trajectory that brought the Mongols to the southern Caucasus, and before that Turkish to Turkey, and before that Iranian languages to ancient Persia and northern Mesopotamia, and still earlier Armenian, Hittite and its sisters, and other early IE dialects to Asia Minor.

These three trajectories are intended as schematic identifications of the main arenas of linguistic spreading. Reality was of course more complex, and in particular the forest and steppe routes are unlikely to have been completely discrete (for instance, the attested ranges of the Baltic and Slavic families straddle the two trajectories). The routes are presented as three discrete trajectories for purposes of hypothesis raising.

Linguistic advances along the southern trajectory appear to have differed somehow from those along the steppe trajectory, since (as is laid out in more detail on pp. 235ff.) movement along the steppe trajectory causes a language

family to cover the entire steppe and make inroads into Central Europe, while movement along the southern route seems to have produced linguistic islands (like modern Turkish and Armenian or ancient Hittite). That is, movements along the steppe route absorbed or displaced previous languages over the entire steppe (and, subsequently, much of Europe as well), while movements along the southern route did so in central Asia but not west of the Caspian, where absorption was less complete and pre-existent languages have tended to survive, especially in the south Caucasus. Furthermore, the IE entry into Europe was by a relatively homogeneous group of dialects with lexical and grammatical sharings (Northwest IE: Meillet [1922: 33ff.] 1967; Ancient European: Gamkrelidze and Ivanov [1984] 1994), while even in the Bronze Age the IE languages of Anatolia and northern Mesopotamia were more diverse: Anatolian (Hittite, Luwian, etc.), (Proto-) Armenian, Phrygian, and others. It may be that PIE was more diverse in its southwestern portion, and the diversity at the western range of the southern route simply reflects greater initial diversity. Or it may be that there were more separate early spreads into Anatolia than into Europe. It may also be relevant that for the steppe route the magnet at the western end was an entire ecosystem – the longer growing season, more abundant rainfall, and hence richer grasslands of the western steppe and the Danube plain – while the magnet for the southern route was the urban centres of the ancient Near East. A successful linguistic entrant to Europe would have been a group drawn by the agricultural possibilities, and prepared not so much to rule as to better the indigenous inhabitants at all aspects of their economic life and hence to trigger ethnic shift and language shift; while a successful entrant to Anatolia would have been prepared to conquer and rule but not necessarily to colonize the entire rural economy at all levels. Despite these differences, all three of these routes will be regarded here as representing the same type of phenomenon, the language trajectory.

As Figure 10.2 shows, loanword and language trajectories need not coincide; they are orthogonal in principle, and sometimes they can go in opposite directions. In the southern and eastern Caspian area, for instance, the spread of agriculture, livestock domestication, and Mesopotamian cultural influence – and presumably loanwords – and the standing trajectory of linguistic spreads go in opposite directions.

Neither a loanword trajectory nor a language trajectory is necessarily a trajectory of substantial population movement. Here they will be treated as routes of diffusion and used to model the vectors of language spread in the abstract, with no reference to human migration. As an example, consider the spread of the Romance language family in Europe. A Roman governing minority brought Latin speech to the towns of Western Europe, and gradually the majority indigenous population shifted to Latin. The Roman conquest of Western Europe was military, and it devastated Gaulish society and exterminated or enslaved entire Gaulish towns, but it did not depopulate the countryside and repopulate it with Romans. Population movement (of Romans into the provinces) occurred but was not great, and population

replacement (of Gauls by Romans) was negligible. Hence the speakers of most present-day Romance languages are the biological descendants primarily of Celts, Dacians, etc. and not of Romans, though their languages are descendants of Latin. The modern distribution of Romance speech will be viewed here as the result of a language spread, not an expansion or migration of the Roman population. One language spread might well be almost invisible biologically, but when repeated language spreads follow the same trajectory they can be expected to make a cumulative significant biological contribution. Therefore – to shift from abstract linguistic-geographic modelling to the description of biological reality – it is probably futile to seek biological reflexes of the IE spread, but eminently worthwhile to seek biological reflexes of long-standing language trajectories in the Eurasian spread zone.[1]

Spread zones can be defined as having few language families, extensive territorial ranges for languages and for language families, generally shallow genetic connections among widely spread languages, and a pattern of language succession whereby one language or family replaces another in space every few millennia. Spread zones are to be expected at high latitudes and in dry and/or seasonal continental interiors, conditions under which population density has generally been low. (Some of the economic and geographic causes of spread zones are discussed in Nichols (1992: 13ff.); Robb (1993) proposes a correlation between population density and genetic proliferation of languages.) In the terms used here, a spread zone is an area where the range of a typical language family is extensive, substantially more extensive than the locus, and language succession occurs when a language from the periphery is drawn into the locus and spreads out over approximately the same range as the previous language or family occupied. The central Eurasian grassland is a good example of a spread zone, and offers the additional advantage of having been well studied archaeologically and linguistically and having over a millennium of its own written history and up to three millennia of partial history recorded by the sedentary empires ringing its southern periphery. Therefore it is relatively easy to define a standing pattern of range, locus, and trajectory for central Eurasia for the last several millennia, and the identities of the last several major spreading languages are known.

The notion of spread zone gives a way of estimating the range of a prehistoric language family from the locus: if the locus was in a spread zone, the range was extensive, hence the daughter languages can be expected to have spread widely. In contrast, if the protolanguage was in an area like the Balkans or the Caucasus, locus and range are largely coextensive and no particular spread is to be anticipated. Indeed, to my knowledge there is no historical or reconstructible example of a language family having spread widely from an area like the Caucasus or the Balkans prior to the development of empire. Languages spread into these areas and hence over time a variety of languages can accumulate there, but such places have never been the staging areas for major linguistic spreads.

A last theoretical consideration involves the distribution of typological diversity in a language family. The typological features I use to describe diversity

include those that can be shown to be most resistant to change and hence most consistent in language families (Nichols 1995). For these relatively stable features there is of course some drift within language families, increasing cumulatively over time. Accelerated change in type can be assumed to result from language contact. Therefore the most pronounced typological divergences are to be expected in the daughter languages at the periphery of a family's range, since those languages distanced themselves from the centre earlier and/or, as frontier languages, have had more opportunities to interact with typologically exotic languages. (See Dryer 1997 where it is shown that the greatest changes in word order occur in languages most distant, in time or geography, from the centre.) The dialect-geographical reality of structural changes at the periphery can be established in the absence of any precise hypothesis about the nature of language contact (substratum? superstratum? borrowing? mixture?), and this is what will be done here.

The next sections use this model to reconstruct the IE homeland and the mechanism of IE language spread.

THE INDO-EUROPEAN HOMELAND: RANGE

Historical parallels and a reconstruction

The central Eurasian grassland has been a spread zone throughout history and recent prehistory. Let us review the historical and prehistoric distribution of languages in the region, beginning with the most recent picture. In the early Middle Ages, Mongolian political and economic rule spread radially from Mongolia, extending westward along the entirety of both steppe and southern trajectories; some Mongolian linguistic spread had begun to follow when the Mongolian expansion to the west was stopped by European military victories in the late Middle Ages.

In the early centuries of this era, languages of the Turkic family spread from the eastern steppe periphery to cover all of central Asia, the entire steppe to the Danube plain, and Anatolia. Figure 10.3 shows the Turkic and Mongol spreads as of the Middle Ages, when the Turkic spread was complete and the Mongol spread was underway. At present, western steppe Turkic remnants from the steppe spread extend to the Crimea (or they did, until the Soviet deportation of the Crimean Tartars in 1944; they extended into the northern Balkans in the seventh and eighth centuries), and remnants along the southern route extend as far west as the southern Balkans and Anatolia (where Turkish is now spoken). Turkic languages have now mostly been replaced by Russian or Ukrainian on the steppe.

Prior to the Turkic expansion, at the beginning of the Iron Age, Iranian spread from somewhere in the vicinity of Bactria, Sogdiana, and the eastern steppe to cover most or all of western central Asia and the entire steppe, much of the Near East at least to eastern Anatolia, and, at least intermittently, the Danube plain, where Slavic vocabulary and ethnonyms attest to

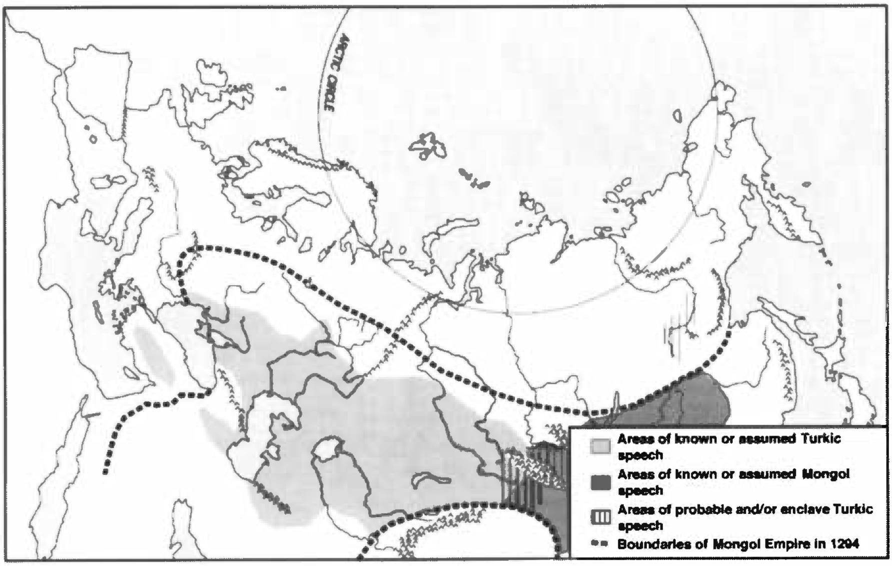

Figure 10.3 Turkic and Mongolian language families, end of thirteenth century AD

a major Iranianization at about the fifth century AD, and where there is good archaeological evidence of a Scythian presence in the mid-first millennium BC (for the linguistic evidence see Trubačev 1967). Figure 10.4 shows the Iranian range as it was in Scythian times.

Prior to the Iranian expansion, in the early Bronze Age, IE spread to cover the entire steppe and the Danube plain (and subsequently all of Europe), with substantial speech communities also in Anatolia (Hittite and congeners) and northern Mesopotamia (surviving in Armenian) and, in all probability, coverage of much or all of western central Asia (probably by ancestral Indo-Iranian). What is historically attested of the IE spread fits closely the pattern followed later by Iranian, Turkic, and Mongolian.

Approximately every two millennia, then, there has occurred a spread of a language family from a locus in the eastern part of the central Eurasian spread zone to cover the steppe and central Asia, extending partially or intermittently to the Danube plain, Anatolia, and northern Mesopotamia. The loci of the historically attested spreads are near the edge of the spread zone rather than in the centre of it: the piedmont to the south (Bactria-Sogdiana) for Iranian, the north of Mongolia for Turkic and Mongolian. The trajectories of language spread run east to west along the steppe and through the desert to the Near East as shown in Figure 10.2.

To take clear and historically well-attested examples, the locus, trajectories, and range of IE must have been much like those of Iranian or Turkic. Figure 10.5 shows the probable PIE locus and range. The placement of the locus specifically in the vicinity of Bactria-Sogdiana is justified in Nichols (ch. 8, Volume 1).

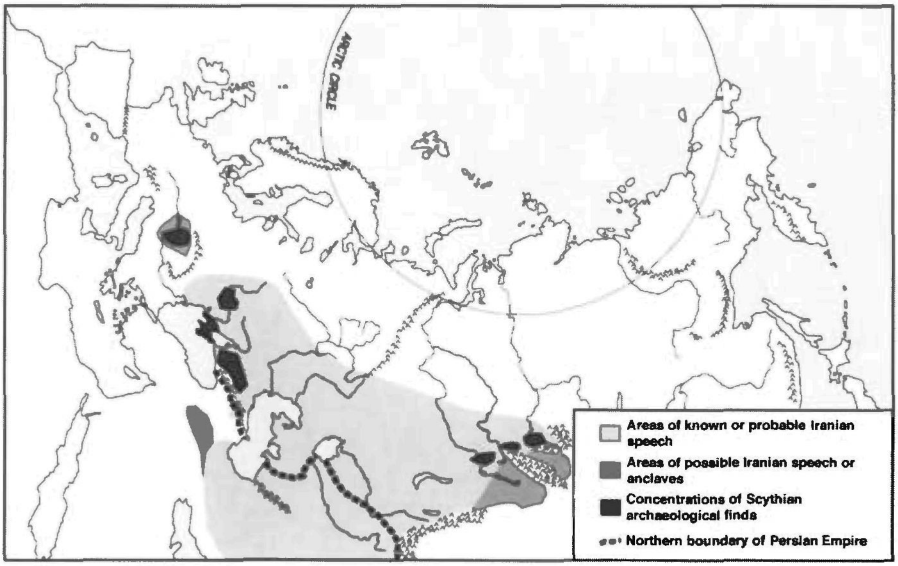

Figure 10.4 Iranian language family, early first millennium BC

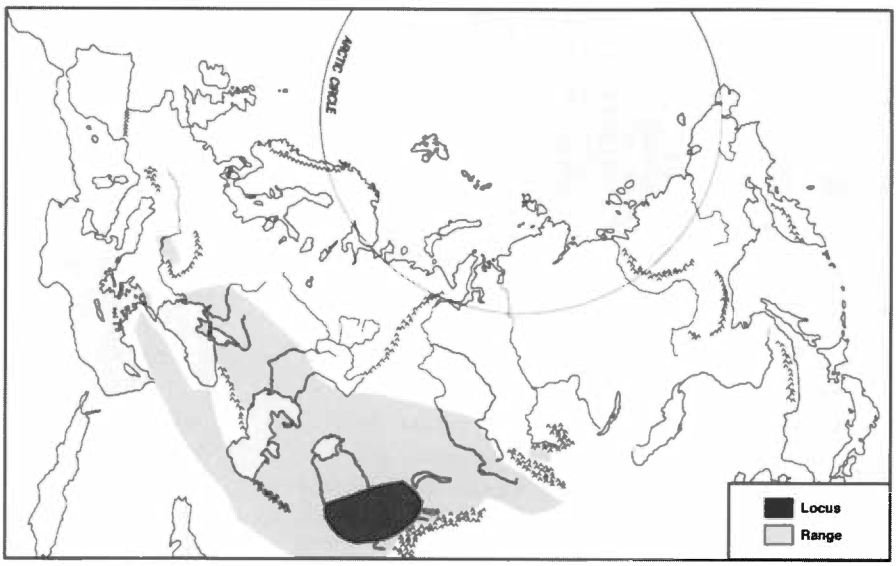

Figure 10.5 Locus and range of Proto-Indo-European

Implications for linguistic palaeontology

IE homeland studies so far have had to resolve the dilemma of how to reconcile conflicting lexical evidence about the IE homeland. Were the Indo-Europeans pastoralists or agriculturalists? The lexical evidence can be used to support both viewpoints (for a summary and argument in favour of agriculture see Diebold 1992). If they were a people of the dry grasslands, how do we explain the presence in their language of words for 'beaver', 'birch', and 'oak', the latter with extensive mythic and cultural salience (Friedrich 1970: 129ff.)? If they were steppe pastoralists, how do we explain the presence of words for 'double door' and 'enclosed yard or garden' suggestive of dwellings in the urban Near East (Gamkrelidze and Ivanov [1984: 741ff.] 1994: 645ff.)? If they were nomadic herders of the plains, how is the presence of a word for 'pig' explained?[2] A homeland reconstructed as locus, trajectory and range removes the dilemma: a locus in the vicinity of Bactria-Sogdiana implies a spread beginning at the frontier of ancient Near Eastern civilization and a range throughout the steppe and central Asia, following the east-to-west trajectory, with occasional or periodic spreads into the Danube plain and Anatolia. The PIE ecological and cultural world, then, included the forested mountains southeast of the Kazakh steppe, the dry eastern steppes, the Central Asian deserts, the urbanized oases of southern Turkmenistan and Bactria-Sogdiana, the eastern extension of the urban Near East, the rich grasslands of the Black Sea steppe, the southern edge of the forest-steppe zone and the Siberian taiga, fresh-water lakes, and salt seas (the Aral and Caspian). The economy of the Indo-Europeans included dry-grasslands pastoralism, settled farming, mixed herding and farming, and trade, including not only trade between farmers and herders in central Asia but also, importantly, control of the antecedents to the Silk Route and the trade connections with India to the south. This economic and ecological diversity is reflected in the vocabulary of PIE.

Some comparisons

In received view, the IE homeland was on the Pontic steppe, and migrations brought IE languages from there to Anatolia, central Asia, and the Indian subcontinent. Mallory (1989: ch. 2) posits intrusion of the Hittites from the northwest of Anatolia, via the Balkans; a later wave of migrations from the Balkans bringing in the Armenians; an Indo-Aryan invasion of India from the northwest; and an eastward expansion of IE on the steppe with Tocharian at the frontier. In a new reconstruction, Gamkrelidze and Ivanov ([1984] 1994) posit a primary IE homeland in the south Caucasus or northern Mesopotamia followed by migration of most dialects to a secondary homeland in the western steppe. Neither of these sets of migrations is posited here; the range and trajectory account for all of the locations of the daughter branches of IE. Tracing the linguistic geography of the daughter languages is essentially a matter of determining which trajectory each daughter branch

followed and in approximately what order. This means that an essentially complete linguistic-geographical reconstruction can be given for a daughter language even when there is no information on the external history of the language. For Hittite, for instance, we can reconstruct a prehistory consisting entirely of generic events: early spread along the desert trajectory, with the language 'taking root' in urban centres near the periphery of the spread, eventually to displace the language previously dominant there. This generic prehistory requires generic archaeological and historical confirmation to certify its plausibility, but it does not require specific confirmation in the form of a firm identification of the ancestral Anatolian culture in the archaeological record. Specific identification is of course extremely interesting and highly desirable, but it is not required for a historical linguistic geography.

This is a departure from previous homeland research, which implicitly assumes that specific identification of a candidate archaeological culture is a requirement and that specific migration histories need to be posited for PIE and for the daughter branches. In other respects, however, the homeland reconstruction given here is broadly consistent with both the received view and that of Gamkrelidze and Ivanov. The received homeland on the steppe is consistent with the range and the major trajectory of the homeland as reconstructed here. Gamkrelidze and Ivanov reconstruct a homeland which in abstract form is similar to the estimate given here. In their view, the PIE homeland lies within a broad range extending from Anatolia through Northern Mesopotamia to the south Caucasus and south Caspian, and within this range it has a more precise location specifically in the south Caucasus. This way of describing location is somewhat like my notions of range and locus. The ranges of the Gamkrelidze-Ivanov homeland and the one posited here overlap, and my locus lies within their range. Thus the model proposed here in some sense subsumes both the received and Gamkrelidze-Ivanov homelands, although taken by themselves without such a model those two are contradictory.

A consequence of the reconstruction offered here is that the attested distribution of IE then turns out to be no singularity but just one regular episode in a standing pattern of spreads. When the IE spread is seen as unique, historically or culturally unique factors need to be posited to account for it: a social and economic organization that encouraged dispersal (Meillet [1925] 1966), the rise of militaristic patriarchalism (Gimbutas 1977, 1991), the rise of agriculture (e.g. Renfrew 1987; Krantz 1988), or wheeled transport and immunity to malaria (Gamkrelidze and Ivanov [1984] 1994). On the approach taken here, nothing novel or unique needs to be posited to account for the IE distribution; it is a natural and regular consequence of geography. Both linguistically and culturally the Proto-Indo-Europeans were absolutely ordinary representatives of their region in their time frame. There was nothing unusual or distinctive, much less unique, about their culture and institutions that must be posited to explain their spread. They did not bring agriculture to Europe, tame the horse, invent patriarchy and warrior cults, or initiate the Bronze Age. They are likely to have had some competitive edge over other steppe

societies, but this could have been, and probably was, a matter of degree and small degree. The main reason why their language spread was that they happened to be in the right place at the right time.

TRAJECTORY

The palaeosociolinguistics of language spread on the steppe

A bare-bones sociolinguistic description of language spread on the steppe can be reconstructed from historical records (see Pritsak 1981, 1982) and ethnographic evidence (e.g. Krader 1963, 1966, 1987), mostly pertaining to the Turkic spread. The entire steppe (and, one can add, the desert as well) tended to use dialects of the same language family as lingua franca. The dialects of that language family were home language and mother tongue for a good many steppe communities, but there were some groups that preserved their original languages while using the steppe language as lingua franca. For instance, the Hungarians preserved their Ugric language throughout a journey of two centuries from east of the Urals to Central Europe. (They also preserved their ethnonym, *Magyar* being cognate to *Mansi*, the self-designation of the western Siberian people formerly also called Vogul in English, whose language is a sister to Hungarian.) They belonged to a Turkic military confederation, and numerous Turkic loanwords in Hungarian testify to Turkic influence on their language and hence to knowledge of Turkic by the ancestral Hungarians. (Abondolo (1987) summarizes the linguistic evidence for Hungarian prehistory; Golden (1990) reviews Hungarian history.) Similarly, though the steppe was mostly Turkic-speaking by the fifth century AD and the Avars seem to have been a Turkic group at their higher levels (Pritsak 1982), the western Avars who interacted closely with the Slavs and may have triggered the Slavic linguistic spread could only have been Iranian-speaking (Nichols 1993). Harmatta (1970) shows that (Scytho)-Sarmatian of the western steppe comprised at least two linguistic varieties. Thus there was a certain amount of linguistic diversity underlying the lingua franca on the steppe. It is probably significant that Hungarian and the Iranian Avar of Central Europe were spoken by groups on the frontier of a new spread, that of Turkic. Once the spread was well underway, the entire steppe seems to have been natively Turkic-speaking.

The steppe had no cities prior to the Middle Ages, but there were urban centres and settled farmers in the oases and mountain valleys of the central Asian deserts; here again there was linguistic diversity behind a widespread lingua franca. Prior to the Russian conquest, Turkic languages were spoken by the nomadic herders of the steppe and deserts and by the rulers of cities, but the settled population was linguistically and ethnically diverse, speaking languages which included survivors from the previous spreading family (Iranian) and those of various immigrants (e.g. Arabic).

National ethnic identity and explicit association of ethnic identity with language seem not to have been usual among the native speakers of the

dominant steppe language. Nor do there appear to have been national or ethnic identities among the settled populations of central Asia (a regional summary is given in Barfield 1994). Among the nomads of central Asia, kinship defined ethnicity at all levels. Clans and clan confederations were the main operative kin-based organizing groups. At higher levels, the entire people claimed descent from a single, generally mythic, ancestor. The collateral lines of descent from that ancestor had different hierarchical positions in society; for instance, among the Iron Age Scythians, the nobility traced their descent to the oldest son of the mythic ancestor (Khazanov 1983: 142, referring to Herodotus). Thus advantages and disadvantages could accrue to various descent lines, and there were mechanisms for changing lines of descent, again at all levels. Illustrating lower-level change, Krader (1966) describes voluntary sub-clan realignment among the modern Mongols:

> On occasion, segments of patrilineages left their native pastures and joined other, more distantly related villages. Such fusions were subject to careful regulation: application had to be made to the proposed host group, which if it agreed and received the support of its clan chief, would accept the newcomers. A fictional set of kinship relations was then introduced; the newcomers became younger brothers, cousins, and nephews of the hosts, and their women and children were denominated accordingly as nephew's wife, brother's child, etc. The economic, social, legal, and religious sanctions were at first applied casually, 'as though' the newcomers were kin; but after two or three generations the former strictures were forgotten.
>
> (Krader 1966: 459)

At the higher levels, clan and tribe realignment could also be brought about top-down, as when the Mongols reorganized the steppe Turks around Mongol leaders, all of whom could claim kinship to Genghis Khan:

> The Mongols destroyed the social organization of the Turkic-speaking nomads who are regarded as the principal ancestors of the Kazakhs; they reshuffled the subdivisions among those nomads and caused considerable ethnic regrouping in the Eurasian steppes.
>
> (Khazanov 1983: 141)

Pritsak (1981, 1982) speaks of clans whose military and economic fortunes enabled them to attract other groups as *charismatic*. This term lacks either cultural or sociolinguistic substance and will therefore serve as a linguistic-geographic technical term for the abstract propensity of the language of an economically and socially prestigious group to spread in space. The real-world elements of charisma were military and economic success, social and ideological attractiveness, and social mobility and ease of assimilation (summarized in Anthony (1995) without using this term). The linguistic-geographic consequence is language shift. Charisma and language shift were mediated and distributed through descent lines, facilitated by lack of language loyalty on

the part of accreted groups, who abandoned their mother tongues to nativize the lingua franca. Language spread of this kind is slow and gradual: the Hungarians did not shift from Ugric to Turkic speech even after over 200 years on the Turkic steppe, and the steppe remained mostly Turkic-speaking even after four centuries of Mongol rule.

Though kinship favoured language shift in practice, descent was orthogonal to language in principle. Krader (1963: 193) notes: 'Clans bearing common names recognize an affinity with each other regardless of whether they belong to the same Turkic people or not.' That is, clan lines continue across language boundaries on the steppe. This appears to have been the case in aboriginal Siberia as well: Vasil'ev (1980) mentions some Samoyedic clan names that continue across language and even language family boundaries. This orthogonality is presumably the result of language spreading in progress. It suggests that, at the height of a spread, language boundaries and the boundaries of archaeologically recognizable cultures may not always coincide.

Further evidence for this orthogonality comes from the spread of Mongol speech across the steppe following the establishment of Mongol rule. The collapse of Mongol power froze the process for recorded history and revealed a language shift underway: there were some groups with Mongol ethnonyms and a Mongol-speaking elite minority, and some Turkic-speaking groups with Mongol ethnonyms.

> In the Eurasian steppes in the post-Mongol period Turkic-speaking tribes would often consist of an original Mongolian-speaking core, onto which separate subdivisions of Turkic-speaking tribes adjoined. In time only the appellations of the tribes remained Mongolian. Such, for example, is the history of the well-known Konghrad (Kunghrad) tribe of Uzbeks. Nevertheless, all members of a new unit traced their descent to one common ancestor.
>
> (Khazanov 1983: 141)[3]

The modern Uzbek take their ethnonym from the name of a great ruler of the Golden Horde of southern Russia (Öz Beg; ruled 1313–41); yet the Uzbek speak a Southeast Turkic language, while the language of the Golden Horde was Kipchak, or Northwest Turkic, and the Golden Horde was militarily and politically Mongol. Mongol speech did not spread to everyday usage among the Kipchak Turks, and Kipchak speech did not spread to the Uzbek. A comparable example from an earlier spread is the modern Bulgarians, whose language is that of the Slavic majority of the southeastern Balkans, while their ethnonym is that of the Bulgar Turks whose rule spread to the Balkans in the seventh century. In all of these post-Mongol cases, the shift to Mongolian ceased after the collapse of Mongol power; in the case of the early Bulgarians, the local Slavic population failed (or perhaps ceased) to shift to Turkic after Bulgar Turkic power fell on the steppe.

The mobility of nomadic peoples helps maintain large linguistic ranges, but it is at best a minor factor in language shifts and hence in language spreads. A

nomadic group's annual cycle of movement from pasture to pasture is stable and does not in itself constitute either migration or expansion. Nomadic pastoralists, unlike sedentary farmers, can and do change habitats when forced to do so by diseases of flocks, local droughts, war, or other emergencies, but these are extreme situations that create refugees, who are not a regular vector of language spreads. At most, normal nomadic mobility might have led to intermingling of languages on the frontiers of spreads, and mobility of refugees might have created speech islands at the periphery of the spread zone.

Even when a spread was complete, more varied conditions continued to obtain at the periphery of the spread zone. Steppe languages could be stranded at the periphery when power shifts occurred on the steppe, whereupon either the local majority language would come to predominate or the language of a new charismatic group would begin to spread from the steppe. When Kipchak Turkic spread on the steppe and covered the previous range of the Bulgar Turkic speech which had had spread there earlier, the Bulgar enclave in the Balkans was stranded and consequently lost its charismatic power and its language began to be replaced by the local Slavic speech. When the collapse of Mongol power stranded Mongol elites on the steppe, their language began to yield to that of the Turkic-speaking majority. These are cases of stranded minorities whose languages yielded to local majority languages, reversing the direction of language shift. There are cases that differ from this typical picture: for instance, the ancestral Hungarians, stranded in Central Europe, abandoned the Turkic lingua franca not for a local language but for their own ethnic language, which they still retain; and the stranded Kalmyk have preserved Mongol speech for several centuries on the post-Mongol steppe. Kalmyk contributes to linguistic diversity at the western steppe periphery. Hungarian, alone of all these examples, is a case of an exotic language imported from the steppe taking root and thriving in a sedentary society.

At least at the western periphery, there was linguistic and dialectal diversity on the steppe itself: Iranian languages differentiated and survived into Turkic times in the west, and the European branches of Indo-European probably differentiated from each other there. In addition, the periphery offered conditions for the sedentarization of steppe languages. Economic refugees who had lost their flocks and hence their capital sometimes settled in nearby farming communities, where they doubtless created linguistic islands. Language spread from the steppe to settled populations must have been favoured by the economic conditions that generally hold between pastoral nomads and their sedentary neighbours. Pastoral nomads, though usually wealthy, are not economically self-sufficient; they need such things as agricultural products which can only be obtained from sedentary peoples (Khazanov 1983). Trade is a vital necessity. In addition to trade, steppe nomads from Scythian times to the Middle Ages have held some adjacent sedentary societies as tributaries, oppressing then economically but otherwise allowing them full independence (Khazanov 1975: 157ff.). In central Asia, nomadic groups have typically ruled the settled population, among whom their languages have gradually taken

root. These are forms of economic dependence (of agriculturalists on nomads), and they must at least sometimes have facilitated the spread of the rulers' languages and institutions. Such spreads probably proceeded without kinship realignment. They must also have proceeded more slowly than spread on the steppe, for the central Asian cities generally remained multilingual and preserved Iranian-speaking communities after the countryside had become exclusively Turkic-speaking.

Even on the steppe at the height of a spread the rate of spread was slow, and at the periphery there were reversals and other local perturbations. The available evidence for social organization and linguistic attitudes in central Asia and on the steppe comes from Scythian times and later, and during most of this time the steppe and desert peoples have formed states. The rate of language spread is likely to have been slower for pre-state peoples, the degree of dialect or even language differentiation in the spread zone consequently greater, and the range less, but for purposes of modelling I assume that the basic mechanism of language spread obtained for the pre-state steppe peoples as well, at least since the Neolithic and possibly earlier.

This section has offered a shift-based model of the trajectory and an abstract sociolinguistics that describes language spread on the steppe and at the steppe periphery. This model does not explain the standing east-to-west directionality of language trajectories in Eurasia, however. That seems to have primarily geographical causes. The climate on the steppe changes gradually from well-watered and temperate at the far west to dry and very seasonal, with long and very cold winters, in the east. The western steppe is tall-grass prairie (or was; now it is mostly cultivated); the eastern steppe is short-grass prairie verging on desert, and central Asia to the south is mostly artemisia desert. The further west, the higher the carrying capacity, in population density of both people and livestock, of the land. In the east, the low carrying capacity made nomadism a necessity; in the west, sedentary agriculture was quite successful once the technology for ploughing prairie soil had developed.

Evidently, economic and ethnic survival in the east required strong social organization and large-scale and highly cohesive groupings like clans, and was favoured by militarism. The social structure required for simple survival in the east was more than enough for survival, and ample for conquest and domination, in the west. Hence any eastern group that attempted to extend its connections westward was likely to succeed, all other things being equal. Though language spread on the steppe was not primarily demographic spread, some migration took place, and westward migration had better prospects for success than eastward migration: easterners moving west would find their territorial needs reduced and their living conditions easier than they were accustomed to, while westerners moving east would need more space and would face harsher conditions than they were accustomed to. A powerful eastern clan, taking in a temporarily down-and-out more westward group, would be in a good position to see its language and kinship network spread, as it would have acquired both subordinates and access to superior pasture. The climatic differences were too

gradual and small to be perceived at the local scale, and are unlikely to have influenced conscious choices of alliances. The military elite, who were the usual migrants in cases of empire spread, were of course aware, at least by Mongol times, of the richer land to the far west, but local choices about alliances in peacetime in, say, eastern Kazakhstan had nothing to do with whatever the common people may have known about the pastures of Ukraine. Rather, this was a process of selection that gave a slight edge to westward extensions of one's network and thus guaranteed that, over time, eastern networks and languages would tend to spread westward rather than vice versa.

The palaeosociolinguistics of language spread into Europe

The situation may have been quite different in Europe, to judge from what can be reconstructed about language and ethnic identity for some of the early branches of European IE. 'The Greeks called themselves "Hellenes" and those who did not speak their language *bárbaroi.* That is to say, the Greeks defined their very nationhood in terms of linguistic identity' (Francis 1992: 474). The early Slavs likewise had a self-designation, *Slověne,* defined their nationality in terms of language, and indeed used the word **ję zykŭ,* 'language, tongue', itself to mean 'nationality, ethnicity' (Nichols 1993). For both Greeks and Slavs the concept of ethnicity defined by language was a fairly abstract or general one, and both groups had lower-level, geographically localized subgroups that were also a focus of ethnic identity. At least for the Slavs, higher-level ethnic identity (i.e. as Slav) was independent of territory, although there was a legendary homeland for all Slavs (no longer occupied by any Slavic group by the time of the earliest written records that are our source on the native Slavic theory of ethnicity); the lower-level groupings, however, were defined geographically, were often named for the geography of their territory, and had origin legends involving migrations to their historical territories.

In short, for at least two early IE branches of Europe a complex native theory of ethnicity and a strong sense of ethnic identity can be reconstructed, and both the theory and the identity were based on language. This probably played a role in the spread of Slavic through Eastern Europe (Nichols 1993), or at least it explains what spread: not just a language but an ethnic and linguistic identity. Reconstructing palaeosociolinguistics may give us a grip on the mechanism of linguistic spreads. In this case, it can be hypothesized, a strong and explicit sense of ethnic identity as manifested in language helps the language of one agricultural society spread to other agricultural societies of the same cultural level. This may have been critical for the spreads through Europe, which was already agriculturally developed and well populated by the time the IE frontier arrived there.[4]

Another factor may have been the natural outcome of contact between clan-based and non-clan-based societies. The steppe peoples of historical times have been clan-based, and this is likely to have been the case in prehistoric times as well, since most other peoples of northern Asia are clan-based. The clans are exogamous descent groups, and a stranded sub-population of such

a group would contain only a subset of the clans; this would restrict the choices of marriage partners and might favour intermarriage with neighbouring ethnic groups, which would lead to bilingualism and might therefore favour language shift (especially if the neighbours – Slavs, in the case of the stranded Avars of Central Europe – had a clear sense of ethnic identity). One way or another, stranding of steppe groups in Europe seems to have at least occasionally led to a situation where a local language was combined with the social and economic institutions that had maintained social cohesion and fostered expansion during a spread on the steppe, and the result was the spread of a local language in Europe. The Slavic spread seems to have been the spread of Slavic speech and ethnic identity combined with Avar political and ideological institutions. The Avars of Central Europe would have been, in the technical terminology of Anthony (1990), scouts and some early long-distance immigrants drawn from the steppe by information gained from the scouts. The political and ideological institutions of the steppe which they brought to Central Europe must have included not only military conquest and political expansion but also what Bellwood (1994) calls an ideology of colonization and founderhood, an ideology which motivated scouts to move out and claim previously unclaimed land.[5]

Mechanism for the Indo-Europeanization of Europe

If geography and sociolinguistics can explain the standing east-to-west trajectory of language spread on the grasslands of Eurasia, as just put forth, what explains the spread of the IE languages over Europe? Pastoral nomadism is not pre-adapted for success in sedentary, agricultural Europe, much of which was forested in ancient times and not inviting to pastoralists. The mechanism suggested just above to account for the Slavicization of the Avars and the Bulgars does not predict Indo-Europeanization of Europe by steppe pastoralists, but would rather predict spreads of indigenous European languages under steppe cultural influence. Evidently, however, some intrusions of steppe languages along the Danube plain ended in the survival and further viability of the steppe languages. The most recent example, and the only clear historically attested example, is the migration of Hungarian from the western steppe to its present location in the ninth century. The early Hungarians were a militarized society that conquered and ruled in Europe without losing their language. Hungarian is an eastern Uralic language, preserved by descendants of a southern Siberian group that joined a Turkic confederation and moved westward across the steppe. As already mentioned (p. 235), not only the Hungarian language but also the ethnonym *Magyar* are of Siberian origin, and this shows that the Hungarians retained not only their language but also their ethnic identity. They were a minority enclave in the Turkic spread, and when stranded in Central Europe they abandoned the Turkic language of inter-ethnic communication and retained their own language. This case proves that stranding of former nomads could result in the importation of eastern languages to Central Europe where they took root.

Insofar as the Indo-Europeanization of Europe was achieved by spreads from Central Europe, therefore, it can be described as a two-stage process: first, an IE-speaking steppe group expanded to the Danube plain and preserved its language, as the Hungarians did in historical times; and second, under the impetus of a later steppe power, that language could spread over a wide swath of Europe, as Slavic did in protohistorical times. I suggest this as a general model for the Indo-Europeanization of Europe. It does not posit migrations across Europe, but relies on occasional expansions of steppe power to the Danube plain (which is geographically an extension of the steppe), stranding of speech communities there in the wake of power turnovers on the western steppe, and occasional renewed expansions of steppe power to the Danube plain.

In historical times, extensions of steppe languages to the Danube plain occurred several times: Huns, Avars, and Hungarians all ruled there in the first millennium AD, but of those three only the Avars seem to have triggered a spread (that of Slavic) and only the Hungarians have preserved their language. If this survival rate is typical, it means that only a minority of the entering languages would survive, only a minority would trigger spreads, and only a minority of languages would undergo spreads. It also suggests that viable language entries and language spreads were of roughly equal frequency, or at least entries were not greatly fewer than spreads, and this is supported by the family tree structure of IE. The IE daughter branches of Europe – Italic, Germanic, Celtic, Balto-Slavic – are coordinate branches, not a chain of derivatives. The European branches show lexical evidence of early interaction (Meillet [1922] 1967; a review of the evidence is Gołab 1992: esp. ch. 2), but this is interaction among already established branches and not evidence for a higher branch. If, however, extensions of steppe languages to Central Europe had been rare relative to spreads, spreads from there would have applied to indigenous languages that were themselves the products of previous spreads and the result would be a family tree whereby (say) Celtic was a daughter branch of Italic, Germanic a branch of Celtic, and Balto-Slavic a branch of Germanic. This is not the situation that obtains for the Indo-European languages of Europe; Celtic, Italic, Germanic, and Balto-Slavic are all initial branches of Proto-Indo-European, and only for the sparsely attested Venetic is there much uncertainty about whether it is a branch of Italic or a branch of its own. Celtic and Italic have resemblances that suggest close sisterhood within IE, but neither is a daughter of the other.

Assuming a series of spreads from Central Europe means that a more peripheral location in Europe can be interpreted as indicating an earlier entry. For instance, the Italic family has a peripheral location: it is attested in the Italic peninsula from earlier times, and its neighbours included non-IE languages. In middle Roman times, when Latin had expanded to occupy most of the Italic peninsula and Celtic had spread to occupy Central and Western Europe, the position of Latin was more peripheral than that of Celtic. The spread that brought Italic to the Italic peninsula was therefore earlier than the spread that brought Celtic to just north of the Italic peninsula.

On this reasoning there is evidence of three or four different spreads of IE languages from Central Europe. The first of these is Italic, which even in ancient times survived only on the southern periphery. Earlier, however – perhaps sometime in the late second or early first millennium BC – ancestral Italic must have been spoken over a broad range of Central Europe. There is evidence of early Italic–Slavic lexical interaction, and Martynov (1983) shows that each such Italicism in Proto-Slavic coexists with an inherited IE root in a related meaning, which it can be argued to have displaced from the core semantic domain; see also Gołab (1992: 117ff.), where the evidence is presented and reviewed in English. The usual interpretation of this lexical evidence is that Italic tribes came into contact with Slavs somewhere in eastern Europe as the ancestral Italic-speakers approached or crossed Slavic territory on their way to the Italic peninsula. But if attested Italic is the remnant of an earlier spread, the Slavic contact could have been with the eastern frontier of the Italic spread. In fact, given the basic implausibility of an Italic migration crossing Slavic territory, skirting the Carpathians to the north, then heading southward into Italy, all the while preserving ethnic and linguistic autonomy, I submit that the early Italo-Slavic lexical doublets in Slavic constitute a strong piece of evidence for an Italic spread.

Next there seems to have been a Venetic spread. Here again the only language to leave historical attestation is found on the southern periphery in the form of the Venetic language, modestly attested in inscriptions from north-eastern Italy in the second half of the first millennium BC, either a distinct branch of IE or a daughter or sister of Italic (Beeler 1949: 51). The ethnonym **Venet-* or **Vend-*, **Vent-*, however, is well attested in and around Central Europe, though the people who bore that ethnonym have mostly been Slavicized by the Slavic spread. Pliny the Elder mentions the Venedi, a 'Sarmatian' people of northern Europe (mid-first century AD). Ptolemy (writing *c.* AD 150) mentions Venedai along the 'Venedian Bay', presumably the Baltic coast. Tacitus (AD 98) places the Veneti between Germanic and Finnic tribes. These three sources all antedate the Slavic spread. Jordanes, writing in the sixth century AD after the Slavic spread, equates Veneti with Sclaveni.[6] There was also a group of Gauls in the vicinity of modern Brittany called Veneti by Caesar (Martinet 1986: 111ff.), together with toponymic evidence which Martinet interprets as indicating that some Veneti became Celticized and migrated with the Celtic spread. Taken together, these attestations suggest that the Veneti occupied most of northern Central Europe, i.e. much of the territory of present-day Poland and eastern Germany (this is also where Martinet places them), and were subsequently Slavicized (or Celticized in part).

The Slavic evidence leads to the same conclusion. An East Slavic tribe mentioned in the Russian Primary Chronicle is known as *Vjatiči*, from **vint-* or **vent-* plus the suffix *-ič* found on etymologically opaque tribe names; the structure of this word and the location of the tribe indicate that the *Vjatiči* were assimilated non-Slavs (Xaburgaev 1979), and the usual interpretation of

this is that they were Slavicized Balts. The Balto-Finnic word for 'Russian' comes from **vend-*, e.g. Finnish *venä-läinen*. The Sorbians, Slavic enclaves in eastern Germany representing the remnant of once-extensive northwestern Slavdom, are called 'Wends' and their language 'Wendish' in German. Thus, along the northern frontier of the Slavic spread various peoples call one or another Slavic nationality 'Wend' or the like, but Slavs do not call themselves by that term; the only Slavic attestation is as the root of an etymologically non-Slavic ethnonym. These facts indicate that the pre-Slavic inhabitants of North Central Europe were called **wend-* or **went-* or the like by their neighbours, who continued to call them by the same name after they became Slavicized. For all we know **wend-* or **went-* may have been the Venetic self-designation; or it may not have. Once the Veneti shifted to Slavic speech and Slavic ethnic identity, they also took on Slavic ethnonyms. The area of Slavic spread in the north appears to coincide quite closely with the earlier Venetic range.

Perhaps the Venetic people spoke the language subsequently attested as Venetic, and perhaps they did not, but shifted to this language in Italy much as the north European Veneti later shifted to Slavic speech. If, however, the attested Venetic language represents their own language and is related to Italic, then either a single IE branch, Italic, underwent two separate spreads from Central Europe or the Venetic spread is not distinct from the Italic spread. If the attested Venetic language is not Italic, and/or if it is assumed not to have been the original language of the Veneti, then the case for a distinct Venetic spread is clearer. Thus a definitive answer to the question of whether Venetic is or is not Italic can have consequences for our understanding of the linguistic prehistory of central or northern Europe as well.

Venetic, whatever its linguistic identity, was associated with a speech community or culture group that had an ethnonym and an obvious ethnic identity. The ethnonym **Venet-* /**Vend-* / **Vent-* is at least as well attested as the Proto-Indo-Iranian ethnonym **Arya* (see again note 4), and like the latter shows phonological variety of its reflexes. The wide attestation and remarkable tenacity of the Venetic ethnonym points to a strong and distinctive ethnic identity, which raises the hope that the Venetic spread might be archaeologically visible. (On the other hand the Slavic spread to be discussed on p. 245, now marked by reflexes of an equally tenacious ethnonym, is not unambiguously visible to archaeology.)

The third spread is that of Celtic, which began by the fifth century BC. Celtic speech is historically recorded from various parts of the territory of the Celtic spread, though most of this territory was subsequently Romanized. The locus of the Celtic spread was in western Central Europe, and perhaps because this locus lay in the western part of Central Europe most of the range was in western Europe. Celtic is the first Indo-European branch attested in Britain, far western continental Europe, and the Iberian peninsula, and on the linguistic evidence of typological changes in insular Celtic it was almost certainly at the Indo-European frontier at least in Britain.

The fourth spread was that of Slavic, which began in the fifth century AD and obliterated whatever remained of easternmost Celtic, Avar, Venetic, and perhaps Dacian as well as some languages or branches of Baltic. The Proto-Slavic ethnonym was carried far across Europe by this spread, and survives in the modern ethnonyms Slovincian, Slovak, and Slovene, and the generic ethnonym Slav (all cited in their English forms) as well as more specific local and tribal ethnonyms (e.g. medieval Russian *Slověne*, a northeastern frontier group).

Indo-Europeanization via the forest trajectory

The Germanic languages in early times were confined to the far northwestern periphery of Europe and could be taken to represent the remnant of another very early spread, at least as early as that of Italic. On the other hand, there are other IE languages in northern Europe which did not spread from Central Europe – the Baltic languages – so it is possible that all Baltic Coast peoples, Germanic included, entered directly from northeastern Europe, having spread there on the forest trajectory. On this interpretation, Germanic entered first, the Baltic languages later, and the westernmost Uralic languages (Saami or Lapp, Baltic Finnic) still later. All these languages would have moved westward by gradual expansion along the forest trajectory rather than by rapid radial expansion from a steppe outpost like the Danube plain. I will assume that Germanic entered in this way, via the forest trajectory, because this interpretation clarifies various other issues and raises some very testable hypotheses about the prehistory of Germanic. For instance, it indicates that the earliest neighbours of Germanic must have been the northeastern dialects of IE (Baltic, perhaps Northeast Iranian, perhaps even Tocharian), while its conspicuous northwestern affinities are a matter of secondary accommodation to new neighbours in Europe.

In Siberia and apparently also in northeastern Europe, there is a longstanding tendency for people and languages to drift north gradually, generally by following the major rivers downstream. Northward drift is not, however, an obvious tendency on the steppe. There is one place where northward drift does appear to be a regular possibility: the Middle Volga, where nomadic tribes have sometimes become sedentary, some linguistic diversity has accumulated, agriculture was important at an early date (Golden 1990: 237), and economic and cultural connections with the forest peoples to the north have always been strongly in evidence. (Golden 1990 calls the interaction between Chuvash (Bulgar Turkic) and the Finno-Ugric groups of the region 'symbiosis'.) Not only farming but also beekeeping was important here. The area was an early target and success in Russian eastward expansion, and in the Bronze Age it was an important link in a long belt of archaeologically Baltic lands extending from the historically Baltic-speaking territory of eastern Europe to the Urals (Gimbutas 1963). In the terms used here, this Baltic belt is the forest trajectory made manifest, as it were, in the form of a temporary 'fast lane' where spread was rapid enough to produce archaeological unity from the Urals to Europe.

The middle Volga, then, is a good candidate for a place where some IE tribes may have split from the steppe languages to drift into the northern trajectory and eventually to enter northern Europe at the Baltic coast and thence to move westward. The parting of Baltic and Slavic, which occurred very early in their history and specifically before Slavic entered the European sphere of agricultural and technological culture (Gołąb 1992: esp. ch. 2), is likely to have taken place here, with Baltic taking a frozen early agricultural terminology and probably a primitive agricultural technology through an area whose economic mainstay was hunting and trapping, eventually to meet up again with Slavic in eastern Europe after the Slavic spread. Slavic, however, spread westward along the steppe route, eventually to reach Central Europe and spread outward. The consensus now is that Balto-Slavic is a single branch of Indo-European with an internal split that is early in the history of Balto-Slavic but none the less post-PIE. The morphosyntactic and lexical conservatism of Balto-Slavic, together with some secondary areal accommodation in northeastern Europe that has increased the superficial resemblances between Lithuanian and Russian, the most studied Baltic and Slavic languages, create the impression of strong similarity between the two branches. An early parting at the Volga accounts for the original unity, the early split, and the continued susceptibility to at least some of the same areal influences. Baltic and Slavic underwent the *satem* shift to approximately equal extents, though not always with identical results (as will be mentioned again below), but a later sound change which changes PIE **s* to a more posterior fricative after **k*, **r*, **u*, or **i* affects Proto-Slavic fully but the Baltic languages only partially. This change applies even more extensively in Indo-Iranian and can be assumed to have arisen there. Its stronger application to Slavic indicates that Slavic occupied a southeastern position in early Balto-Slavic, in closer proximity to Iranian. My own suspicion is that Slavic remained for some time to the southeast of Baltic and in the steppe sphere of influence and was drawn into Central Europe during the period of Iranian dominance on the steppe. By this time Baltic speech had already reached Northern Europe.

There is no guarantee that the archaeologically Baltic tribes of the Bronze and Iron Ages (Gimbutas 1963, 1970) were linguistically Baltic, but neither is there any evidence against this. Even if they were not, the Baltic languages have been in Northeastern Europe for a long time, since well before the Slavic expansion and perhaps since before the Venetic expansion. Baltic entries to Europe probably continued over a long period of time, as the attested Baltic languages are rather diverse and their break-up must have begun nearly as early as the split of Baltic from Slavic. On the firm linguistic evidence of river names, Baltic languages probably representing a separate branch of Baltic extended through the upper Dnieper drainage well into central Russia (Toporov and Trubačev 1962). Even the West Finnic languages, the latest entrants along the northern trajectory, arrived before the Slavic expansion (the relative chronology of the Slavic spread in the northeast through Baltic and West Finnic languages is summarized by Kiparsky (1952, 1979: 77ff.)).

All of this points to a very early chronology for the IE spread along the northern route. If Germanic entered along this route, it was one of the first IE languages into Europe, and if it entered the forest trajectory at the Middle Volga, it must have left the IE locus very early.

Languages moving along the forest trajectory seem to have approached and entered Europe and continued to expand westward in Europe without sudden spreads like those that periodically emanated from Central Europe. Only at the far end of the forest trajectory, in northwestern Europe, were there secondary spreads: the Gothic state expanding across eastern Europe, the spread of Viking power in the late Iron Age, the German eastward expansion beginning in the early Middle Ages. The first two were spreads of political and economic power that had surprisingly little lasting linguistic effect, and the third was a simultaneously political/economic and demographic expansion which spread German speech into previously Slavic-speaking territory. None was ancient. Possible archaeological evidence of an ancient spread in this region is the corded ware culture which covered much of northern and Central Europe and (in the terms used here) extended along the entire forest trajectory to the upper Volga around 3200–2300 BC (Mallory 1989: 108). However, the geography of the Corded Ware culture does not require a northern origin; it could just as well represent a cultural spread from Central Europe which extended to northern Europe and from there spread along the forest trajectory. In linguistic terms such a spread might have included a language spread from Central Europe covering northern Europe (geographically similar to the later Venetic spread) and almost certainly included a loanword trajectory spreading European words along with culture elements far to the east. Chronologically, the Corded Ware culture is too early to represent even the earliest IE spread from the Danube plain or a very early Germanic entry. Geographically, the Corded Ware territory is unprecedentedly large for undifferentiated linguistic unity in Europe: Mallory (1989: 145–6) and Anthony (1991: 196) note upper limits on the size of early ethnolinguistic groups, though not with reference to the Corded Ware culture, which they both regard as a possible early IE expansion to Europe. It probably represents the geographical sphere of influence of a cultural magnet that contributed to language spreading along the forest trajectory.

The overall linguistic geography of the forest trajectory is then as follows. This trajectory had an important intake point just west of the Urals, around the middle and upper Volga. Germanic originated among the northeastern dialects of disintegrating PIE prior to the *centum–satem* shift, was among the earliest dialects to spread westward on the steppe, and was drawn into the forest trajectory quite early. During the early Bronze Age it is likely that Germanic speech extended from the upper Volga through the northern forest-steppe and southern forest area of present-day Russia, reaching at least to the northern trajectory terminal in the vicinity of the southeastern Baltic coast (present-day eastern Poland, Latvia, Lithuania, and Estonia) and over time continuing to spread westward across the coastal plain. Perhaps around

2000 BC, and certainly after the *satem* shift had spread to the steppe, the Balto-Slavic split occurred, with Baltic being drawn into the forest trajectory where it spread (presumably by language shift) to replace the eastern dialects of ancient pre-Proto-Germanic.[7] Germanic is now the truncated western periphery of the earlier pre-Proto-Germanic range which must have extended over most of the forest trajectory at one time. The eastern part of the range was swallowed by the Baltic spread, and some of the central part may have been engulfed by the Venetic and Slavic spreads. The earliest sound shift that firmly identifies Proto-Germanic as a distinct branch in Indo-European is Grimm's law, or the first Germanic sound shift, which occurred relatively late, around 500 BC (Polomé 1987), and therefore affected a Proto-Germanic already long resident in Europe.

Present-day Baltic languages are, like modern Germanic, the truncated remnant of a once larger family, and for this we have firm evidence. Medieval Slavic chronicles mention a now-Slavicized Baltic enclave, the Galindians, in what was then northern Russia; Baltic river names are thick in what is now Slavic-speaking territory on the Upper Dnieper (Toporov and Trubačev 1962); East Slavic loanwords in Latvian and Lithuanian postdate Slavic contact with West Finnic, which began only in the sixth century when East Slavic speech spread over formerly Baltic territory to reach Finnic (for this loanword chronology see Kiparsky 1952, 1979: 77ff.).[8]

The languages that are now northwestern in Indo-European – Germanic, Balto-Slavic, Celtic, and Italic – have some lexical stock in common which is partly native IE (though regionally specialized) and partly exotic (Meillet [1922] 1967; Polomé 1990). The usual interpretation is that these languages evolved as neighbours out of an undifferentiated European lobe of PIE and that this entire evolution took place in Europe. On the model proposed here this would be impossible. The four branches arose in different parts of late PIE under different local dialect influences (Italic and Celtic doubtless in the northwest, Germanic and Balto-Slavic in the northeast) and followed different trajectories to Europe (Italic, then Celtic, then Slavic taking the steppe trajectory, Germanic and then Baltic the forest trajectory). Spreads from Central Europe have brought about some contact of Celtic and Italic and of Celtic and Germanic, but this is very different from continuous contact of the entire respective branches from PIE times on. Only Germanic and Baltic or Balto-Slavic are likely to have been in continuous contact.

The Northwest IE vocabulary reflects just this prehistory. There is a stratum of words exclusive to some or all of the four branches, much of it of probable exotic origin, which entered the branches so early that their consonant correspondences make them indistinguishable from native PIE words (that is, they entered before any of the branch-specific consonant shifts arose). The term for the northern domesticate rye, an exclusively Balto-Slavic and Germanic word (e.g. English *rye*, Slavic **rŭžį̆*, Lithuanian *rugys*), can be taken as emblematic of this stratum. There are later words which cannot descend from a single PIE protoform but none the less go back to a single secondary

protoform. An example is pre-Germanic and pre-Balto-Slavic **plōg-* 'plough' (e.g. English *plough*, Russian *plug*), with a **p* : **p* correspondence that cannot be PIE as it postdates the first Germanic consonant shift. Still later words enter Germanic, Baltic, and Slavic separately as indicated by formal variation (e.g. English *silver*, Lith. *sidabras*, Slavic **sĭrebro* 'silver'). These three examples show that the spread of loanwords has a long history as measured against the known chronology of sound changes in the affected languages. In general it is Germanic and Balto-Slavic that figure most prominently and consistently in the relevant vocabulary sets (see e.g. the examples reviewed by Polomé 1990), which supports both the continuous adjacency of Germanic and Balto-Slavic and a long-standing loanword trajectory extending out from Europe along the forest trajectory at least as far as the Volga.

The Balkans

The steppe trajectory has a terminal in Central Europe, from which periodic spreads emanated, but not all languages following the steppe trajectory proceeded to Central Europe. The Balkan Peninsula can be entered from Central Europe, as Slovene, Serbo-Croatian, and Romanian have entered it, and also directly from the Pontic steppe, as the (subsequently Slavicized) Bulgars entered. That is, it is possible but not necessary that languages in the Balkan Peninsula are the result of Central European spreads. (Romanian spread with the Roman Empire, but since that was the first empire in Europe outside of Greece, empire can be ruled out as a cause of pre-Romanian spreads into the Balkan Peninsula.) The Slavic and Romance spreads extend far outside the Balkan Peninsula, and I will assume that this is the normal situation and that therefore, by Occam's razor, additional Central European spreads should not be posited just to account for languages found only in the Balkans. That is, distinct spreads of Greek, Albanian, etc. from the Danube plain should not be added to the known Central European spreads (Slavic, Celtic, etc.). This is equivalent to assuming that all IE languages of the Balkan Peninsula that came from the north, except for Slavic and Romanian, entered directly from the steppe and not from Central Europe.

The Balkan Peninsula can also be entered from Asia Minor, as Turkish and Romany have entered in historical times. For no ancient language of the Balkans do we have evidence as to whether it entered from Asia Minor or from the steppe to the north. There are circum-Aegean cultural sharings (e.g. Minyan grey pottery, found in both Greece and western Anatolia from *c.* 2300 BC), but no clear evidence for directions of spread: Mellaart (1971a, 1971b) associates Minyan ware with Anatolian linguistic influence on Greek and posits an entry to Greece from Anatolia and ultimately brings Hittite to Anatolia from the east; Palmer (1980: 9–26) follows Mellaart; Crossland (1971, 1988) posits an Anatolian entry to Anatolia from the Balkan Peninsula). Greek itself is generally believed to have entered from the north (e.g. Crossland 1971, 1988), with an initial entry before the first attestation of Mycenean Greek at *c.* 1400 BC (and, on archaeological evidence, perhaps as early as

2100). Gamkrelidze and Ivanov ([1984: 898ff.]1994) argue for a Greek entry from Anatolia, on the evidence of grammatical and lexical sharings between Greek and other southern languages (Indo-Iranian, Armenian).

Around 1000 BC the urban civilization of coastal Greece was invaded by Dorians, speakers of a Greek dialect who came from the mountainous regions of northwestern Greece and today's southern Albania and southwestern Macedonia. The Dorians defeated and displaced the Achaeans and their dialect displaced the dialect of the Achaeans. Most sources take these Dorian invasions as a second wave of Greek entries and a model for the Hellenization of Greece. They may be a useful local model, but they shed no light on the trajectory by which Greek entered the Balkans, since their point of origin, though north of Greece, is still far south in the Balkan Peninsula and is the point where trajectories leading in from Anatolia, from the western steppe, and from the Danube plain all coincide.

Greek has substantial non-native vocabulary, some of it obviously of indigenous pre-IE Mediterranean origin (Hester 1964; Morpurgo Davies 1986). Attempts have been made to link some of this to non-Greek IE, either Anatolian (e.g. Palmer 1980: 9–26, following Mellaart 1971a, 1971b) or an otherwise unattested Balkan or Central European branch variously called Pelasgian or Prehellenic (Hamp 1990 and references therein). The lexemes in question are reviewed by Hester (1964). Hamp's work on this vocabulary is the most rigorous and systematic (he traces it to a general Central European vocabulary also visible in the Northern European branches and having to do with agriculture, swineherding, beekeeping, and related activities alien to the steppe and desert), but even so the material merely bears rather than demands the analysis as IE and from a single IE branch.

In short, there is no compelling evidence, either linguistic or archaeological, for bringing Greek to its attested location either from the north or from Anatolia. I provisionally assume an entry from Anatolia because, within early IE, Greek finds some ancient lexical and grammatical affinities with Armenian and Indo-Iranian, a fact which suggests spread along the southern trajectory. These affinities include the past-tense augment and some shared tree names; for the latter see Friedrich (1970).

The other early IE languages of the Balkans are conventionally listed as Macedonian, a divergent dialect of Greek or its near sister, just to the north of Greek; Illyrian, along the Adriatic coast; Thracian, to the east of Illyrian; and Dacian, north of Illyrian. Albanian, now in the territory of ancient Illyria, is attested only in modern times but has ancient Greek loans and an extreme Balkan structural type suggesting long presence in the Balkan peninsula. In ancient sources 'Macedonian', 'Illyrian', 'Thracian', and 'Dacian' are ethnonyms or geographical terms, so there is no a priori reason to assume that any of them was a single or discrete linguistic entity. A personal name from Illyrian territory appears to be *centum*: *Vescleves*, a cognate to Gk. *Euklees* and Skt. *Vasus'ravas,* from PIE **wesu-k'lewes* 'of good fame' (cited from Langslow 1988). This word cannot be from ancestral Albanian, since Albanian is a *satem*

language. Therefore, I assume that the linguistic population of the ancient Balkan Peninsula was diverse, as the modern population is, and contained at least one *centum* language ('Illyrian') and one other language ('Thracian') in addition to ancestral Albanian (*satem*). Messapic, sparsely attested in southern Italy, may also belong with Illyrian and may therefore have entered Italy from the Balkan Peninsula.

If all these ancient Balkan languages entered from the northeast, then Greek was the first one in, followed perhaps by Illyrian, then Thracian, then Dacian. This chronology is based on the early attestation of Greek (in the form of Mycenean in the mid-second millennium BC), peripheral location, and simple distance from the entry point. If any of the other ancient Balkan languages entered from Asia Minor, the best candidates are the southern ones – Greek, Illyrian, and Thracian – and those with early coastal orientations – Greek and Illyrian. I provisionally assume northern entries for all but Greek, because this raises some highly testable hypotheses about Greek.

This section and the preceding ones have described the Indo-Europeanization of Europe in terms of two simple abstractions: entry at one or another trajectory terminal, and spreading from Central Europe. These mechanisms are very different from those of language spreading on the steppe. Again we see that an adequate, albeit generic and minimal, linguistic-geographical description of the prehistory of a language can be achieved by answering just the two questions of trajectory terminal and chronology of spreading from Central Europe. For instance, to arrive at an adequate generic account of the prehistory of Germanic we need only evidence that it entered Europe via the forest trajectory (or, what may be the same thing, evidence that it separated early from its original neighbours and only later, though still early in prehistory, came into a northwest European linguistic context). For Greek the basic issue is whether it entered the Balkan Peninsula from Anatolia or from the western steppe. These questions in turn can be answered with linguistic data and do not require archaeological identification of particular cultures as Proto-Germanic or Proto-Greek, though of course archaeological identification is to be welcomed.

Asia Minor, Northern Mesopotamia, and South Asia

Most entries to Asia Minor and northern Mesopotamia have presumably come from the east via the desert trajectory, as Turkish and its sisters have entered in historical times. There is at least the possibility of entry from the Balkans, since ancient Greece colonized the Aegean coast of Asia Minor. Herodotus reports an oral tradition to the effect that the Phrygians came from the Balkan Peninsula, though this cannot be verified linguistically. The major early languages of Anatolia and northern Mesopotamia are, in west-to-east order of their earliest attested locations, Greek (coastal colonies), Phrygian, Anatolian, Armenian, Indic (in the form of Mitannian Aryan), and Iranian (Old Persian). Turkic languages enter after Iranian, beginning in the ninth century AD. The west-to-east ordering can be regarded as an approximate relative chronology

of ancient entries. Phrygian is attested later than Anatolian, but this does not mean it arrived later. In view of its location in western Anatolia, Phrygian is a very early entrant regardless of whether it came from the Balkans or from the east.

There has been only one major IE entry to India, that of Indic, which came from the northwest in the early second millennium BC. Two other branches of Indo-Iranian are also found in the northwest: the Nuristan languages, in the mountains of northwestern India and northern Pakistan, and Iranian languages, in Pakistan, Afghanistan, and Tadjikistan to the north. The relative chronology for entries to India is therefore Indic, then Nuristani, then Iranian. There have been major invasions of India from the northwest in historical times, notably those of Alexander the Great in the fourth century BC and the Mongols in the Middle Ages, but these have not resulted in language spreads. Indic, in contrast, has spread to cover half of the Indian subcontinent. The aboriginal Dravidian languages are reduced to islands in the northwest, where Indic dominates, but still cover large continuous areas in the southeast, farthest from the Indic entry point.

Though many questions remain concerning the chronology, mechanism, and sociolinguistics of the spread of IE to Anatolia and to India, at least it can be said that there is no clear reason to posit for either of these regions a series of spreads like those in Central Europe. For each of them there appears to be just one trajectory and one entry point.

PROTO-INDO-EUROPEAN DIALECT GEOGRAPHY

This section describes the IE spread in terms of an advancing front with peripheral features, a rate of advance, and isoglosses behind it. Table 10.1 shows an approximate and tentative ordering of IE entries at the four trajectory terminals. Also shown is the *centum* or *satem* nature of each language family, if known.

The earliest known IE entrant anywhere is Anatolian, attested (in the form of Hittite) at the head of a kingdom in the nineteenth century BC and on archaeological grounds likely to have entered Anatolia in the mid-third millennium (Mellaart 1971a; Mallory 1989: 28). Apart from this early appearance, no entrant to Europe, no Central European spread, and no Balkan, Anatolian, or Indic entry of IE needs to be posited before the second millennium. Most entries or spreads of IE branches occur during the second millennium BC, a few during the first millennium BC, and only one (Slavic) later than that. The Turkic spread began in the early centuries AD, and viable Turkic linguistic spreads reached the Pontic steppe and the Near East beginning around the seventh century. The last IE entry in the north, Baltic, is followed by West Finnic (a branch of Uralic), and if the break-up of West Finnic linguistic unity coincides with the Finnic entry to northern Europe, it need not have occurred before about 1 BC/AD.

Table 10.1 Approximate timing of entries at four trajectory terminals

Time: *Millennium:*	*Very early* *Third*	*Early* *Second*	*Middle* *First* BC	*Late* *First* AD	*Totals* *(IE branches)*
Trajectory: Northern	(none)	Germanic^{K} Baltic?^{S}#	Baltic?^{S}#	(Finno-Ugric)	n = 2 K = 1 S = 1
Central Europe	(none)	Italic^{K} Venetic?^{K}#	Venetic?^{K}# Celtic^{K}	Slavic^{S}%	n = 2.5 K = 2 S = 0.5
Balkans	(none)	Greek★^{K}% Illyrian^{K} Thracian?# Albanian?^{S}#	Thracian?# Dacian Albanian?^{S}#	Slavic^{S}%	n = 5 K = 1.5 S = 1.5
Anatolia	Anatolian^{K}	Greek★^{K}% Phrygian Armenian^{S} Indic^{S} Iranian?#^{S}	Iranian?^{S}#	(Turkic)	n = 5.5 K = 1.5 S = 3
Totals:	n = 1 K = 1 S = 0 S = 0% (0%)	n = 9 K = 4 S = 3.5 S = 39% (47%)	n = 4 K = 1 S = 1.5 S = 37.5% (60%)	n = 1 K = 0 S = 1 S = 100% (100%)	n = 15 K = 6 S = 6 S = 40% (50%)

Notes

K = *centum* languages, S = *satem* languages. (Languages with no superscript are too poorly attested for their *centum/satem* status to be known.) Language names in parentheses are non-IE. The chronology for Central Europe is for spreads; others are for entries. ★ = trajectory ambiguous; ? = chronology ambiguous. In the Totals column at the bottom, languages listed in each of two columns or rows are counted as one-half in each. Venetic is not counted since it may belong with Italic. The percentages are *satem* languages: first as per cent of total entries, second as per cent of known *centum* + known *satem* languages. # = shown twice in same trajectory; % = shown twice in same column.

Frontier phenomena

Some of the IE languages on the western frontier give evidence of having interacted with non-IE indigenous languages. Greek has a stratum of non-IE vocabulary that is presumed to be indigenous Mediterranean (as discussed on p. 250), and at an early date Greek already exhibits one of the diagnostic features of the Balkan linguistic area: a definite article. For the ancient Germans, whose history begins later, we have no record of non-IE neighbours to the north or west, but the Germanic lexicon attests non-IE substratal vocabulary particularly well (Polomé 1986, 1990), and a hallmark of Germanic syntax is the cross-linguistically unusual and non-PIE V2 rule. Hittite likewise has a grammar and sound system that are superficially but markedly transformed from the IE original, presumably under non-IE influence. The

conclusion to be drawn is that Germanic, Greek and Hittite were frontier languages, the first IE languages in at the ends of their respective trajectories. The Celtic languages of Britain also underwent profound and probably contact-induced grammatical change, and Irish in particular is highly un-European in its structural typology (Gensler 1993; for the word order see Dryer 1997). What Celtic seems to have encountered in Britain was an indigenous language that was not IE nor even remotely European in its structural type (Gensler 1993). See Jackson (1955) on the rather clearly non-IE nature of the Pictish language of Scotland and the definitely non-IE nature of Pictish institutions. Thus there is evidence of peripheral type shift all around the western periphery of IE. Balto-Slavic, the only branch with little or no non-IE contact until relatively recently, is also the most conservative branch.

Chronology of the Indo-European dispersal

For the Turkic languages, only some 500 years elapsed between the beginning of the spread in the early centuries AD and the first viable linguistic entries in the west. The Turkic spread across the steppe surely proceeded no more slowly than the IE spread, but there is no evidence that it proceeded significantly faster. The Turks were mounted nomads among mounted nomads. The Indo-Europeans are likely to have been mounted nomads, at least in the western part of their range: the horse was domesticated and bridled by 4000 BC on the western steppe (Matyushin 1986; Telegin 1986; Anthony and Brown 1991; Anthony 1995). Thus the IE spread probably involved mounted nomads among mounted nomads in part, but – depending on the exact time frame for horseriding in the eastern steppe – may have involved, in some regions, mounted nomads spreading among people who did not ride, and this might have meant a greater cultural and political differential and hence perhaps a faster spread. In Central Europe it took Slavic speech only about a century to spread to the Baltic coast and the southern Adriatic, and the Slavs, though they had domesticated horses, were not nomads and were generally not mounted. The distance from western Mongolia or eastern Kazakhstan to Central Europe is about six times the distance from the middle Danube to the western Baltic coast or the southern Adriatic. Moving at the Slavic rate, then, IE languages could have traversed the steppe in about six centuries, and this is consistent with the speed of the Turkic spread. Even if this rate of spread is halved in order take a more conservative stance, it would have taken only about a millennium for a language to spread across the entire Eurasian steppe.

Therefore, on linguistic-geographic grounds, even if the IE spread began in the easternmost reaches of the steppe it need not have begun earlier than about 3500 BC in order to account for the earliest archaeologically likely entry of Anatolian to Anatolia even at the slower rate of spread. Let us now consider what age can be given to PIE on linguistic evidence. One way of estimating age is glottochronology, which generally yields reasonable approximations to known or inferred reality when enough daughter languages can

be compared. The standard glottochronological examination of IE is Tischler (1973: 97–104) which gives dates for PIE ranging from 2400 BC to 4200 BC (for different pairwise comparisons of daughter languages), with a mean and median both at 3300 BC. (Taking the average of all these pairwise figures is justified because the comparative method has already told us the structure of the IE family tree and we know all pairings are tree-wise more or less equivalent, with the possible exception of Anatolian.)

A standard, if informal, way of measuring the age of language families is comparing them to the Romance languages, since many linguists at least in the western world have enough familiarity with Romance to use that family as standard in an intuitive assessment of the degree of lexical and grammatical divergence of other families. Romance is about 2,000 years old, and the extremes of its grammatical and phonological divergence are represented by Romanian and French. The earliest attested IE languages are Mycenean Greek, Hittite and Vedic Sanskrit, all attested in the mid-second millennium BC. (Vedic Sanskrit is actually first attested later than this, but represents the language of about 1000 BC.) Measured in terms of typological parameters, their divergence seems on the whole not much greater than that of, say, vernacular French, an Italian dialect, and Romanian. Like French and Romanian, Hittite and Vedic Sanskrit have undergone very different changes, probably representing contact influence. A distant-Romance-like degree of closeness between these three ancient languages suggests that PIE broke up not much more than 2000 years before their attestation, hence sometime around 3800–3300 BC.

A third way of dating protolanguages is comparing the reconstructed vocabulary to datable archaeological or historical phenomena. The PIE vocabulary includes a developed terminology for livestock, including not only sheep and goats, the earliest domesticates, but also cattle, pigs, and – importantly – the horse (first domesticated around 4000 BC); there is also a terminology for wheeled transport (first known in Mesopotamia in the late fourth millennium and in Europe not before 3300–3100 BC: Piggott 1983; Anthony 1991, 1995) and a word for 'plough'. There is a word that may have meant 'ore' and must have referred to copper, but there is no word for 'bronze' or 'iron'. The PIE inventory of tree names is consistent with what, on palaeobotanical evidence, was to be found on and near the steppe in the Atlantic period (*c.* 5000–3000 BC) (Friedrich 1970). (General summaries on the IE vocabulary include Gamkrelidze and Ivanov [1984: vol. II] 1994: 377ff.; Watkins 1985; Mallory 1989: 110ff. *passim*). The lexical reconstructions point to a time frame that is well post-Neolithic, pre-Bronze Age, specifically Eneolithic, and no earlier than the fourth millennium.

These three different means of estimating the age of a family converge to give IE a break-up date between 4000 and 3300 BC, perhaps around 3700. As Anthony (1991, 1995) emphasizes, the dispersal and loss of the speech community could not have antedated the appearance of wheeled transport at 3300–3100 BC, for cognate terminology for 'wheel', 'yoke', etc. is found in all daughter branches.

The spread of steppe influence to Central Europe is described by Marija Gimbutas, who in a number of publications has argued that Europe underwent a series of waves of invasion or influence, emanating from the western steppe via Central Europe, that brought prototypically IE institutions such as patriarchy, militarism, and a sun deity to Europe, eventually destroying the very different earlier civilization. The first such wave begins about 4400–4300 (Gimbutas 1977, 1991: 361ff.), the second at about 3500 BC, and the third at about 3000 BC. If the IE break-up began about 3700 BC on structural and linguistic-geographic grounds and loss of community was no earlier than 3300 BC, and if the first IE spread from Central Europe, that of Italic, occurred not much before 2000 BC, then Gimbutas's first wave is much too early to have been IE. The second wave is barely within the time-frame of the IE break-up, but break-up and entry to Central Europe are different things. Even the third wave is earlier than would be required to account for the first Central European spread. The only possible conclusion, if one accepts Gimbutas's notion of three episodes of influence, is that the Indo-Europeans were not the first carriers of steppe culture to Europe. Assuming that some language spreading accompanied the steppe influence, the earliest IE spreads from Central Europe were not the first language spreads from there. In that case the westward trajectory antedates IE, and the first languages to move along it were not IE.

Family tree structure and wave propagation in a spread zone

It was mentioned above that the IE daughter languages that spread from Central Europe do not join together to form an intermediate branch (Italo-Veneto-Celto-Slavic). Rather, each spreading language seems to have entered Central Europe separately. (If Venetic and Italic form a single branch, there are still three very distinct groups: Italo-Venetic, Celtic, Slavic.) Similarly, the languages at the ends of the other trajectories do not form branches such as Greek-Illyrian or Anatolian-Armenian. The striking feature of the IE family tree is the early, almost simultaneous spread of many branches from a single root. The earliest partial branchings are those of Balto-Slavic and Indo-Iranian, and these are not bifurcations of IE but bifurcations of individual daughter branches.

Any branching in IE that took place before the spread would have a very different distribution. The Turkic family gives evidence of what it would look like. Proto-Turkic split first into Bulgar (or Oghuz) and Common Turkic, and now only Chuvash (of the Middle Volga) represents Bulgar while numerous languages and dialects from the Balkans to eastern Siberia represent Common Turkic. The branching of Common Turkic is multiple and simultaneous (and shallow, almost on the order of a dialect continuum). The initial bifurcation and the multiple Common Turkic branching are easily evident to comparativists. The initial bifurcation took place before or as the spread began, so that the first Turkic language to spread westward, ancestral Bulgar, was already differentiated from its sister, ancestral Common Turkic.

The differentiation of Common Turkic occurred after and as a result of the spread. If PIE first split into Anatolian vs the rest (let us call the rest Common IE on the analogy of Common Turkic), its history is parallel to that of Turkic. First Anatolian split off and moved away (analogous to Bulgar); then a minimally differentiated Common IE spread, diverging into daughter branches only after and as a result of the spread.

Reasoning on the analogy of Turkic history, we can assume that an undifferentiated PIE (or Common IE) is what spread and first covered the entire range, and that the differentiation into daughter branches took place at the periphery of the range. A differentiation that began before the spread would have yielded an initial bifurcate branching of IE analogous to that of Bulgar vs Common Turkic. Furthermore, to judge from the distribution of the two deep branchings, Balto-Slavic and Indo-Iranian, a pre-spread differentiation would probably make itself felt along more than one trajectory. As discussed above, Baltic and Slavic are likely to have entered Europe along two trajectories, Baltic drifting into the forest trajectory at about the Volga and Slavic continuing along the steppe route. The history of Baltic and Slavic thus indicates that a sufficiently ancient branching produces a sub-family that is distributed over more than one trajectory.

The Indo-Iranian facts point in the same direction. The bifurcation of Indo-Iranian is well known to be evident not only in South Asia, where all three of Indic, Nuristani and Iranian sub-branches are found, but also in ancient eastern Anatolia, where either an Indic language or undifferentiated Indo-Aryan or Indo-Iranian is evident in early Mitannian vocabulary (e.g. *aika-wartanna* 'one course', where *aika* is cognate to Sanskrit *eka* 'one', an Indic word) while Old Persian and Avestan are Iranian (for the Mitannian words and their interpretation see Burrow 1959: 27ff.). Along the forest trajectory as well there is evidence of either an early Indic presence or undifferentiated Proto-Indo-Iranian or Proto-Indo-Aryan. Among the Indo-Iranian loans into early Finno-Ugric are some so phonologically archaic that they could well be Proto-Indo-Iranian (Burrow 1959: 24ff.; Abaev 1972: 28), e.g. Votyak *suzer* 'younger sister', Mordvin *sazor*, Cheremis *sõzar*; cf. Sanskrit *svasar*, but Avestan *xvaÑhar*, Iranian **hvahar* 'sister'. Iranian, but not Indo-Aryan, regularly reflects PIE **s* as *h*, so this Finno-Ugric form looks more Indic than Iranian.[9] Abaev also cites some less well attested forms that could be specifically Indic, e.g. Vogul *tas* 'alien', cf. Skt. *dasa-* 'barbarian, demon'. These borrowings would have taken place somewhere in the vicinity of the southern Ural Mountains. They were received from a steppe language and incorporated into Finno-Ugric as it began its spread along the forest trajectory. This linguistic evidence for an Indic or Proto-Indo-Iranian wave preceding Iranian on the steppe is weak but legitimate. In partial confirmation of it, Kuz'mina (1994) identifies the Andronovo culture of eastern Kazakhstan in the mid-second millennium BC as Indo-Aryan.

There is also evidence for Indo-Aryan along the steppe trajectory in the form of a set of Crimean place names which Trubačev 1977 identifies as

Indo-Aryan. This evidence is even weaker – place names in general have poor diagnostic value since they lack denotational meaning – but carefully researched and again legitimate. If Trubačev is right there is evidence for an Indic advance to the western steppe. Taken together, the Finno-Ugric and Crimean evidence are consistent with the assumption of a short-lived Indic or Indo-Aryan presence at the frontier of the Iranian spread on the steppe, in addition to the well-known Indic frontier in northeastern Mesopotamia and India.

These cases indicate that a sufficiently early split shows up along more than one trajectory, and it follows that any development within the PIE locus should be evident along all three trajectories. This is precisely what happens with the *centum–satem* division. Table 10.1 shows the relative chronology of *centum* and *satem* entries to the west. Along each trajectory, *centum* languages precede *satem* languages, and the frontier languages, those most clearly showing peripheral type shift, are *centum*. In the north, Germanic (*centum*) enters first, followed by Baltic (*satem*). In Central Europe, Italic, Venetic and Celtic are *centum*, and only the last entry, Slavic, is *satem*. In the Balkans, the picture is muddied by ambiguity about the direction from which languages entered and poor attestation which makes it difficult to judge the *centum/satem* type of the ancient languages; above the fragmentary 'Illyrian' evidence was interpreted as suggesting the presence of a *centum* language in the west. Greek, wherever it came from, was a frontier language, and it is *centum*. In Anatolia, the first attested IE branch is Anatolian, which is *centum*; Phrygian gives evidence of some *satem* reflexes; and Armenian, Indic and Iranian are all *satem*. *Satem* languages enter fairly early in the north and in Anatolia, while in Central Europe their appearance is delayed until the spread of Slavic long after IE entries along the other trajectories have ceased.

In view of its attestation along all three trajectories, the *centum–satem* split must have arisen in the eastern part of the range, in or near the locus. The *centum–satem* split did not produce a branching in IE, but at most a dialectal isogloss. Since in the west the *satem* languages are the later entrants wherever the chronology is clear, and the frontier languages are *centum*, the *satem* side of the isogloss must have been to the east. Since *satem* languages are most numerous in the south, while *centum* languages predominate in the north and the *centum* language Tocharian appears far to the east of the locus as well as somewhat to the north, the *satem* side of the isogloss must have lain not just to the east but specifically to the southeast. Since the *centum–satem* division is visible along all three trajectories, it arose in or near the locus after the spread began and spread outward after the *centum* languages had begun to spread. For this reason it behaves in dialect-geographical terms as though it were an innovation, although in structural terms it is difficult to see either the *centum* type or the *satem* type as innovative relative to the other; they are simply two treatments of the inherited consonant system.

CONCLUSION

Recognizing the Eurasian linguistic spreads as a general process gives us a natural periodization of linguistic life on the Eurasian grasslands and their periphery. The obvious breakdown is into the language families that spread over the whole range and occupied it for a time. Thus we have the pre-IE period, the IE period, the Iranian period, the Turkic period and the Mongolian period (which did not progress beyond the early stage). In reality there may have been more than one pre-IE spread, so that in reality there may have been more than one pre-IE period, but we will probably never find evidence of their distinct identities, so the single label 'pre-IE' is the best that linguistics can offer. The periodization is a relative chronology, and the results of comparative linguistics and archaeology can give it an absolute chronology as well: the pre-IE period begins around 5000 BC, the IE period *c.* 3500–3000 BC, the Iranian period *c.* 2000–1500 BC, the Turkic period in the first centuries AD, and the Mongol period in the Middle Ages.

The chief utility of this periodization is that is makes it possible to assign linguistic phenomena to periods without making – or needing to make – unduly precise claims about absolute chronology, archaeological cultures, or precise linguistic provenance. For instance, linguistic spreads in Central Europe can be periodized in this way: the Italic spread occurred in the IE period, the Venetic spread either then or in the Iranian period, the Celtic spread in the Iranian period, the Slavic spread at the very end of the Iranian period. If the corded ware culture (discussed on p. 247) represents a language spread from Central Europe, it was a pre-PIE spread, and the northern loanword trajectory with which it was probably associated was also a pre-PIE phenomenon. Linguistic entries to Europe can be periodized in the same way: the Slavic entry to Central Europe was in the Iranian period, the Italic entry was in the IE period, and the period of the Celtic entry remains to be determined; the Germanic entry in the north was during the IE period, the Baltic entry during the Iranian period.

It should similarly be possible to attach regional vocabulary loans to linguistic periods. English *goat* and *buck* and their cognates and Nakh-Daghestanian resemblants (see Nichols, ch. 8, Volume I)) are now found only at the far fringes of the western steppe, in western Europe and the Caucasus. Both borrowings must have taken place early, when an ancestral European language passed by the Caucasus or while the IE source language was still within the sphere of influence of a western steppe power. They therefore belong to the IE or early Iranian period. (Note that tracing a word to the Iranian period does not mean it is Iranian in origin, for neither of these words is Iranian, and *buck* and its European cognates have unambiguously non-Iranian *centum* consonants. Iranian is likely to have been only an intermediary.) Turkic *buɣa* 'bull' spread across the steppe with the western Turkic languages (beginning in the early centuries AD) and has been borrowed into nearly every lowlands Caucasian language as well as Ukrainian and Russian. (It generally displaces

the native term to a sense such as 'ox'.) This is the kind of distribution that the *goat* and *buck* sets are likely to have had when first borrowed. *Buɣa* 'bull' spread during the Turkic period, as is clear from its etymology, but there are others of the same period whose sources are less clear. Russian *sobaka* 'dog' and *sorok* '40' are loans from the Turkic period whose specific sources are at best dubious if sought within Turkic (see Vasmer 1964; Trubačev 1967; Hamp 1974). Like other borrowings of the Turkic period, they are not Proto-Slavic but only East Slavic, found only in languages from the Slavic–Turkic frontier.

This is a programmatic chapter which has left many facts out of the picture in order to sketch the outlines of a linguistically adequate model of language spreading in the IE area. It rests on four new proposals: the notions of locus, range and trajectory, including the difference between language trajectories and loanword trajectories; language shift as an important mechanism of language spreading in spread zones; the dynamic of spread zones, including the accumulation of diversity at the periphery rather than near the locus; and periodic episodes of language spreading from the Danube plain as the primary mechanism for the Indo-Europeanization of Europe. These pieces yield a model which shows the IE spread to be a manifestation of a regular phenomenon in Eurasia, makes it possible to offer adequate generic prehistories of IE daughter languages by tracing them to particular trajectories and entry points, and makes it possible to use the evidence of ancient loanwords while recognizing that languages have moved in space and even the earliest attested locations are not necessarily points of origin for languages. Much of the argument has involved showing that various known phenomena fit this model at least as well as any other, and often better. The Indo-European spread was part of a regular process whose ultimate conditions lay in geography. Hence an answer to the question of where the Indo-European homeland was located also answers the question of how and why the family spread as it did.

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NOTES

1 The impression gained (by me in any case) from reading sources such as Ammerman and Cavalli-Sforza (1984), Renfrew (1987), Gamkrelidze and Ivanov [1984] (1994), and Piazza *et al.* (1995) is that the biological aspect of the Indo-European spread (and/or the spread of food production and neolithic economy) is seen as a matter of progessive dilution of the Indo-European (or early farming) genetic heritage as Indo-Europeans (or early farmers) spread into new territory and intermarried to some extent with previous inhabitants; that is, Indo-European has a describable biological identity that undergoes gradual statistical change. In the approach taken here the spread is viewed as occasional small contributions to indigenous gene pools which have describable biological identities. That is, the history of Indo-European is not one of expansion, movement, dilution, and cumulative small changes in a biological population; rather, it is a history of small and mostly local interactions within a large-scale biological continuum whose overall gradient is not changed by language spread, though local and regional steepness can be affected by a long-term trajectory of language spread.

2 It is not the mere presence of individual words like this in Indo-European that has been seen as problematic. After all, a society that does not raise swine itself can still have a word for them. The vocabulary in question is numerous and well installed in Indo-European and is therefore taken to indicate not just acquaintance with the referents but close cultural involvement with them.

3 On the other hand, Krader (1963: 193, 238, *passim*) regards Mongol ethnonyms among Turkic peoples as having primarily symbolic value and being perhaps historically fictive:

> The clans were groups which usually bore the name of an eponymous ancestor, or recreated the name of an illustrious Turkic or Mongol group of the past days of glory. In both instances, whether through reference to an individual (mythical or actual) or to a group, the consciousness of unity over the course of history, and its affirmations, were asserted. . . . Certain names, primarily the most glorious, recur again and again among the Uzbeks, Kazakhs, Karakalpaks, and Kirgiz, [and] formed a small reservoir continually tapped.
>
> (Krader 1963: 193)

Though clan names may indeed have had only this symbolic value in recent times, I assume they reflect original historical unity.

4 Though ethnic identity and pure ethnonyms are not typical for attested and reconstructible steppe and desert nomads and are apparently not necessary for language spread on the steppe, they have occurred. An example is **ārya-*, an ethnonym attested as Skt. **ārya*, Ossetic *Iron* (ethnonym for speakers of one dialect, and name of the dialect), Scythian *Alan*, Persian *īrān* 'Iran'. Since this term is attested in both Indic and Iranian and at both the southeastern and northwestern periphery of the trajectory, it arose in the locus at the beginning of the spread. Though probably also attested in Celtic (Irish *Eire*) it is chiefly Indo-Iranian, and can be reconstructed as an ethnonym either for an important part of that society (a charismatic clan?) or for the entire ethnic group during the spread. Together with their ethnonym the Indo-Iranians evidently carried a distinctive and prestigious religion with elaborate ritual including poetic composition or recitation, so the Indo-Iranian spread was a simultaneous spread of language, religion and ethnic identity. This is quite different from the later Turkic and Mongol spreads (both Turks and Mongols were notable for their religious tolerance), which show that a language spread on the steppe and desert could proceed without entailing religious and ethnic identity.

5 Previously unclaimed, that is, by anyone of the scouts' social group and allies. A myth to the effect that land newly claimed by scouts or colonizers was previously uninhabited can be reconstructed for the late western steppe Iranians on the evidence of a Slavic and Chechen (Nakh-Daghestanian) parallel. Váňa (1983: 29–30) reports the origin legend of the Czechs: 'At that time the surface of this land was covered with broad expanses of desolate forests, without a single inhabitant. . . . When men entered these deserted places, . . . seeking suitable places for human dwellings. . . .' A similar motif appears in a Chechen legend to the effect that prior to the Late Middle Ages the forested belt of the Chechen piedmont was uninhabited, and when the Chechen first settled there the wild animals had never before seen humans (Vertepov 1892: 82; Maksimov 1893: 40). In reality, of course, the land to which Iranian colonizers spread – Central Europe, the central Caucasian foothills – was far from uninhabited.

6 All these references come from secondary sources.

7 The *satem* shift applies inconsistently in Balto-Slavic. Some words with PIE palatovelars show plain velar reflexes in Balto-Slavic, e.g. Slavic **korva*, Baltic **karvē* 'cow', Slavic **gord-*, Baltic **gard-* 'city, wall', etc. (beside Russian *zorod* 'haystack, enclosure for haystack', Lith. *žardas* 'structure for drying heads of flax'); some are inconsistent (e.g. Slavic **gǫsi*, Lith. *žąsis* 'goose'); and some are consistently *satem* (e.g. Slavic **zolt-*, Baltic **želt-* 'gold', Slavic **sŭto*, Baltic **šimtas* '100'). Explanations for the variation include a *centum* substratum (Gołab 1992: 76ff.) and phonological conditioning for the *satem* shift (Gamkrelidze and Ivanov [1984: 112] 1994: 96). A more parsimonious explanation is that the *satem* shift was lexically gradual and that Balto-Slavic preserves a frozen picture of the sound change in progress, with frozen doublets, variation, and discrepancies between Baltic and Slavic and some indications (less than fully consistent) of phonological conditioning. As a sound change that is lexically gradual but phonologically abrupt, the *satem* shift corresponds to a dominant view of sound changes in contemporary historical linguistics (see e.g. Chen and Wang 1975; Labov 1981).

8 This was the first contact with a non-Indo-European language for Slavic. Exclusively IE neighbours for the first 4,000 years after the PIE break-up explain the remarkable linguistic conservatism of Slavic. Likewise, though early Baltic was at the northern frontier of IE and had a long history of contact with Finnic, the surviving Baltic languages were insulated from Finnic contact by now-extinct Baltic languages until probably the first millennium AD, which explains their conservatism.

9 Phonologically the Finno-Ugric word could have come from PIE itself, but this is chronologically unlikely. The Iranian block of words in Proto-Finno-Ugric is well known and much studied, but there is no good evidence of direct borrowing from PIE into Proto-Finno-Ugric. When a word (like that for 'sister') crops up that looks PIE, the most conservative assumption is to go only one step beyond the (early or Proto-)Iranian that is known to have contributed vocabulary to Proto-Finno-Ugric.

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11 *Gimbutas' Kurgan–PIE homeland hypothesis: a linguistic critique*

KATHRIN S. KRELL

INTRODUCTION

The Eurasian Steppe hypothesis, perhaps better known as the 'Kurgan' hypothesis, was first proposed in 1956, and subsequently developed over a period of three decades, by the Lithuanian-born scholar Marija Gimbutas (1921–94), Professor of European Archaeology at UCLA from 1963 until her retirement in 1990. Not surprisingly, her hypothesis draws its principal arguments from archaeological research. However, it has been quite influential in the fields of historical-comparative linguistics and comparative mythology, as well as archaeology.

It will be argued here that, although the hypothesis claims to have answered the question of the origins of Proto-Indo-European (PIE), an inherently *linguistic* construct, there are serious problems with this hypothesis from a linguist's point of view. The general criticism is that in constructing her Kurgan hypothesis, Gimbutas places far too much emphasis on archaeological data and on her particular interpretation thereof, while failing to relate that data to current linguistic research on PIE. In particular, it will be shown here that the vocabulary items commonly reconstructed for PIE hardly form the striking correlation with Kurgan archaeological data which Gimbutas claims.

Another inherent weakness of the Kurgan hypothesis is that it relies quite heavily on questionable assumptions. For example, Gimbutas assumes that language spreads and behaves in a way parallel to the material aspects of a given archaeological culture; in this particular context, she simply supposes that all the early Kurgan-type cultures spoke the same language (or at least closely related dialects). A further assumption crucial to her hypothesis is that language, in this case late PIE, can be (and indeed was) imposed on entire local populations by a small conquering warrior elite. Finally, she seems to assume that the PIE vocabulary reconstructed to date can in fact be used at face value to adequately represent a PIE culture and environment at a specific point in prehistory.

Gimbutas' avoidance of the actual PIE data, coupled with her unfounded assumptions concerning language change in general, question the credibility of her overall argument.

GENERAL OVERVIEW OF THE HYPOTHESIS

Gimbutas first identified the Indo-European people with what she coined the 'Kurgan' archaeological culture in a 1956 monograph, *The Prehistory of Eastern Europe, Part I.* The term 'Kurgan' is the Russian word for 'barrow', and was chosen by Gimbutas as a cover term for particular groups of prehistoric cultures which, according to her, attest to a common cultural tradition. Gimbutas chose 'Kurgan' because the principal trademark identifying this tradition is said to be the construction of earthen barrows as part of the burial ritual (Gimbutas 1963: 821).

According to Gimbutas' research, the Chalcolithic (i.e. copper-stone or post-Neolithic) Kurgan culture first developed into its characteristic form some time during the third millennium BC in the Eurasian steppes; more precisely, in the steppe-lands which extend from the lower Volga basin north of the Caspian Sea, around the Sea of Aral, through much of (northern) present-day Kazakhstan, all the way to the Altai mountains, and as far east as the Sayan mountains and the upper Yenisey river. Thus Gimbutas posits quite a vast territory as the PIE homeland.

This geographically enormous homeland is justified, under Gimbutas' theory, by the fact that the early Kurgan culture appears to have been a semi-nomadic, pastoral peoples who depended on large pasture-lands to sustain their sheep- and cattle-based economy. Furthermore, she claims that the Eurasian steppes constitute 'the area where horse-breeding must have first occurred' (Gimbutas 1963: 820), presumably by the Kurgan people, for whom the domesticated horse would certainly have had many advantages.

According to Gimbutas, by the time the Kurgan people started to expand westward and southward from their Eurasian steppes, they were already divided into several dialectal sub-groups, a view which most linguists would probably agree with, given the vastness of the posited Kurgan homeland. However, she does assume that, originally, all Kurgan-type settlements were occupied by speakers of one common language.

Gimbutas' chronology of the Kurgan culture was somewhat tentative in the theory's early years, and even with the advent of carbon-dating technology in the 1960s, she kept reworking the original chronology, as C-14 testing was slowly being perfected. Ultimately, however, she divided the Kurgan tradition into three stages (Gimbutas 1970: 177):

1. Early Kurgan or Kurgan I north of the Black Sea precedes the infiltration into (Danubian) Europe and Near East.
2. Middle Kurgan is the period of continuous waves of expansion, subdivisible into early and late phases, the *Kurgan II* and *Kurgan III* periods.
3. Late Kurgan or Kurgan IV is the last period of a relatively uniform culture before the crystallization of Bronze Age cultures in Europe.

Gimbutas dates the Early Kurgan period back to the fifth millennium BC, the Middle Kurgan period to the fourth millennium BC, and the Late Kurgan period to the third millennium BC (Gimbutas 1970: 177–81). Furthermore,

she identifies *three separate waves* of westward Kurgan migrations instead of a single one, as she had previously thought (Gimbutas 1965), giving them the following preliminary chronology:

> (a) Expansion to the Balkano-Danubian area in the first half of the fourth millennia BC . . . and subsequently to northern Europe.
> (b) Expansion to Transcaucasia, Iran, and parts of Anatolia during the second half of the fourth millennium BC.
> (c) Raids and/or expansion to the Aegean and Adriatic area and to Syro-Palestine and possibly Egypt around 2500–2200 BC.
>
> (Gimbutas 1970: 191)

In her 1977 article, she pushes back this chronology, settling on the following dates for the Kurgan waves of incursion into Europe:

> Wave No. 1 at c. 4400–4300 BC;
> Wave No. 2, c. 3400–3200 BC; and
> Wave No. 3, c. 3000–2800 BC.
>
> (Gimbutas 1977: 277)

This chronology is upheld in her subsequent articles (Gimbutas 1978, 1980, 1985). For the purposes at hand, the primary focus will be on the Early Kurgan culture and on the significance of its attributed date and location, not only as a concrete prehistoric milieu for PIE speakers, but also as the source of the earliest dispersal (Gimbutas' Wave No. 1) of PIE as a language.

GIMBUTAS' ARCHAEOLOGICAL DATA VERSUS THE LINGUISTIC DATA

General

One of the most avidly used types of archaeological information in trying to solve the question of the PIE original homeland is that data which relate to the physical environment, be it natural or man-made. Gimbutas claims that the archaeological findings which she has gathered with respect to the Kurgan culture correlate with the linguistic picture which has been sketched of PIE. Her confidence on this point is expressed in no uncertain terms:

> Constantly accumulating archaeological discoveries have effectively eliminated the earlier theories of Indo-European homelands in central or northern Europe and in the Balkans. The Kurgan culture seems the only remaining candidate for being Proto-Indo-European: there was no other culture in the Neolithic and Chalcolithic periods which would correspond with the hypothetical mother culture of the Indo-Europeans as reconstructed with the help of common words.
>
> (Gimbutas 1970: 156)

In examining the gist of Gimbutas' archaeological findings, the relevant data seem to revolve principally around the following themes: the Kurgan fauna and flora, a Kurgan pastoral economy (with the domestication of the horse as its revolutionary pivot), and the use of wheeled transport. Gimbutas claims that the Kurgan cultural tradition which she has been able to piece together archaeologically is perfectly reflected in the vocabulary commonly reconstructed by linguists for PIE. Indeed, this is her primary argument for associating the two in the first place.

To evaluate how well the PIE lexicon may actually reflect Gimbutas' Kurgan data, this chapter examines the items she can account for archaeologically alongside those items which have been reconstructed by traditional historical-comparative linguistics. The linguistic data have been taken from four authoritative sources currently available to scholars: Buck's (1949) *Dictionary of Selected Synonyms in the Principal Indo-European Languages*, Pokorny's (1959) two-volume *Indogermanisches Etymologisches Wörterbuch*, Mann's (1984) *An Indo-European Comparative Dictionary*, and Watkins' (1992) IE roots in the *American Heritage Dictionary*.

The accumulated data, Kurgan and PIE, will be juxtaposed and presented in the form of four thematic tables. In these tables, each PIE form reconstructed for a given concept will be accompanied by some of its attested reflexes found in ancient Indo-European languages. Reflexes are not synonyms; they are lexical items which share some sort of semantic link with the proto-form, but, most importantly, they can also be demonstrated as being phonological derivatives of that proto-form. Whenever a reflex is not provided in one or the other cell of a table, it is because the given language or language group possesses no item which is phonologically and semantically traceable to the reconstructed PIE form in question. Of course, this does not mean that the concept itself has no label in that language; it may have some other label, but one which cannot be phonologically traced to that particular PIE root.

For the linguistic data in Tables 11.1, 11.2, 11.3 and 11.4, the following symbols and abbreviations are used:

schwa	ə	OE:	Old English
m—dash:	not found in the data	OHG:	Old High German
(?):	uncertain	OIr.:	Old Irish
Goth.:	Gothic	ON:	Old Norse
Ir.:	Irish	OPr.:	Old Prussian
MIr.:	Middle Irish	Toch.:	Tocharian
Myc.:	Mycenean Greek	Ved.:	Vedic Sanskrit
OBr.:	Old Brythonic	W:	Welsh
OCS:	Old Church Slavonic		

Fauna

The animal remains found by Gimbutas in Kurgan I and Kurgan II type excavations are primarily the bones of (in her opinion) domesticated animals:

horses, cattle, pigs, sheep, goats, and dogs. Most of all she stresses the importance of the domesticated horse, which represents no less than 65 per cent of the animal bones found in any given Early Kurgan settlement, and therefore, in her view, must have been central to the Kurgan culture and economy. She also finds evidence of wild animals (e.g. Gimbutas 1970: 157–60), but although wild animal bones show a greater diversity of species than those of domesticated ones, they typically only account for about a fifth of all the bone remains (ibid.: 157). Nevertheless, Gimbutas implies that all these species are accounted for by the proto-vocabulary.

However, after assessing her claim in Table 11.1, we can see that the correlation between Early Kurgan fauna and PIE fauna is not quite as tight as Gimbutas claims. The donkey, for instance, is not accounted for by any of the PIE sources, while there are several well-attested PIE forms, such the ones for 'duck' and 'goose', which are not represented in the Kurgan data. Admittedly, given the remoteness of the period in question and the distinctive nature of linguistic change, one could hardly have expected a perfect match in any case; nevertheless, a link between the Kurgan and PIE fauna on the basis of Gimbutas' own argumentation already seems less than clear cut.

Like many other scholars, Gimbutas apparently assumes that in PIE society certain animals had already been domesticated. From a strictly linguistic point of view, this cannot necessarily be assumed. We should not presume that in prehistoric times words corresponding to 'horse', 'cow' or 'sheep' carried the same semantic content as in historic times; intrinsic connotations of domestication may be more recent than the period in which PIE was spoken.

Flora

Gimbutas places somewhat less emphasis on the correlation between flora attested within the Kurgan homeland and that of the PIE proto-vocabulary. On the one hand, Kurgan agriculture was 'not highly developed' (Gimbutas 1970: 161), so as to exclude any reference to a variety of grain crops. On the other hand, the local forests were home to diverse species of trees (ibid.: 160). In this respect, once again, the Kurgan and PIE data do not quite overlap, as shown in Table 11.2 below. Not only is there a discrepancy with respect to the trees which can be accounted for, but also it seems that PIE discerns at least three or four different types of grain, not to mention other cultivable plants.

However, the presence of crop and fruit tree names *per se* does *not* entail the practice of advanced agriculture, just as the presence of forms designating certain animals which we now associate with domesticated species does not in itself entail organized animal husbandry. Forms such as ⋆*ăbel-*, ⋆*pūro-*, ⋆*m(e)lēi-*, ⋆*ĝherzd(h)*, ⋆*bhar-*, ⋆*bhabhā* and ⋆*lĭ-no-* (to use Pokorny's reconstructions) may simply refer to wild species which became associated only later with similar, cultivated varieties.[1] Another example can be found in the harvesting of wild rice by native North American tribes. In both cases, cereals became a source of food without being cultivated for this purpose.

Table 11.1 Gimbutas' Kurgan fauna vs PIE fauna

Kurgan fauna	*Latin reflexes*	*Greek reflexes*	*Other European reflexes*	*Sanskrit reflexes*	*Buck's PIE forms (1949)*	*Pokorny's PIE forms (1959)*	*Mann's PIE forms (1984)*	*Watkins' PIE forms (1992)*
horse	equus	ἄπποϛ	MIr. *ech*	açva-	⋆ek̂wo-	⋆ek̂uo-s	⋆ek̂uos	⋆ekwo-
cow	bōs	βουϛ	OE *cu*	go-	⋆g^{w}ou-	⋆g^{u}ou-	⋆guōus	⋆g^{w}ou
cattle	pecus	πέκω	OHG *filhu*	paçu	⋆peku	⋆pek̂-	⋆peu	⋆peku
pig	sūs	συϛ	OE *sū*	sūkara	⋆su-	⋆sū-s	⋆sūs	⋆sū-
young pig	porcus	πόρκοϛ	OE *fearh*	–	⋆pork̂o-	⋆pork̂o-s	⋆pork̂os	⋆porko-
sheep/ewe	ovis	οἄϛ	Ir. *ōi*	avi-	⋆owi	⋆oui-s	⋆ouis	⋆owi-
lamb	agnus	ἄμνοϛ	OCS *agne*	–	⋆ag^{w}(h)no-	⋆ag^{u}h-no-s	⋆agunos	⋆ag^{w}h-no-
goat	–	αἴξ	OE *gāt*	ajā-	–	⋆aiĝ-	⋆aiĝis	–
dog	canis	κύων	Ir. *cū*	çvan-	⋆k̂uon-	⋆kuon-	ku	⋆kwon-
auroch	–	–	–	–	–	–	–	–
deer/elk	–	ελλόϛ	Lit. *élnis*	Toch. *yäl*	⋆el-	⋆elen-	⋆elənis	–
boar	aper	κάπροϛ	OHG *ebur*	varāha-	⋆epero- (?)	⋆epero-	–	–
wolf	lupus	λύκοϛ	Goth. *wulfs*	urka-	⋆wlk^{w}o-	⋆ulk^{u}os	⋆ulk^{u}os	⋆wlk^{w}o-
fox	vulpēs	αλώπηξ	Ir. *sinnach*	lopāça	–	⋆ulp-, lup-	–	⋆wlp-ē-
beaver	–	–	–	–	–	–	–	⋆bhi-bhru-
squirrel	–	–	–	–	–	–	–	–
badger	–	–	–	–	–	–	–	–
hare	cānus	-κασία	OE *hasu*	śaśah	–	⋆k̂as	⋆k̂as-	⋆kas-
marten	–	–	–	–	–	–	–	–
bear	ursus	ἄρκτοϛ	Ir. *art*	rksa-	⋆rk̂^{p}-o-	⋆ŕk̂to-s	⋆ərkt-	⋆rtko-
donkey	–	–	–	–	–	–	–	–
Not in Gimbutas	*Latin reflexes*	*Greek reflexes*	*European reflexes*	*Sanskrit reflexes*	*Buck's PIE forms*	*Pokorny's PIE forms*	*Mann's PIE forms*	*Watkins' PIE forms*
duck	anas	νήσσα	OE *ened*	āti-	⋆anəti-	⋆anət-	⋆anətis	–
goose	anser	χήν	ON *gās*	harisa-	⋆ĝhans-	⋆ĝherzd(h)	⋆gĥansis	⋆ghans-
salmon	–	λακάω	ON *lax*	lasāmi	–	⋆lak-	⋆lak̂əsos	⋆laks-
eel/snake	anguis	ὄφιζ	Li. *angìs*	áhih	–	⋆ang^{u}(h)i-	⋆anguhis	⋆ang^{w}i-
crane	grūs	γήρανοζ	OE *cranoc*	–	–	⋆ger-	⋆gerənos	⋆gerə-
eagle/large bird	vulturis	ὄρνιζ	OHG *aro*	–	–	⋆er-/or-	⋆oros	⋆or-
bee	opis	–	OE *bēo*	–	⋆bhī-	⋆bhei	–	⋆bhei-

A further problem with Gimbutas' literal use of the proto-vocabulary is that it leaves no room for possible, and even probable, but unrecoverable semantic shifts. In fact, any semantic content (i.e. meaning) which may be associated with a phonologically reconstructed form represents no more than an educated guess on the part of linguists. We simply cannot be certain that PIE ⋆*pū-ro* originally referred to the plant we now call *spelt*, and therefore it would be imprudent to limit the possible *Urheimat* homeland only to those regions in which spelt can be attested for the third millennium BC.

Table 11.2 Gimbutas' Kurgan flora vs PIE flora

Kurgan flora	*Latin reflexes*	*Greek reflexes*	*Other European reflexes*	*Sanskrit reflexes*	*Buck's PIE forms (1949)*	*Pokorny's PIE forms (1959)*	*Mann's PIE forms (1984)*	*Watkins' PIE forms (1992)*
oak	dūrus	δόρυ	W *derwen*	*dāru-*	*derwo-	*deru-	*deru-	(*perk^{w}u-)
birch	farnus	–	OE *beorc*	*bhūrja-*	*bherəĝ –	*bherəĝ-	*bherĝos	*bherəg-
fir	–	–	–	–	–	–	–	–
beech	fagus	θηγος	ON *bōk*	–	*bhāgo-s	*bhāgo-s	*bhāĝos	*bhāgo-
elder	–	–	–	–	–	–	–	–
elm	ulmus	–	Ir. *ailm*	–	–	*elem-	*əlm-	–
aspen	–	–	–	–	–	–	–	–
willow	–	–	–	–	–	–	–	–
cherry	–	–	–	–	–	–	–	–
melon	–	–	–	–	–	–	–	–
apple	abulus	–	MIr. *uball*	–	–	*ăbel-	*ăbəlos	–
spelt/ grass (or wheat)	–	πυρός	OCS *pyro*	*pūrah*	–	*pū-ro-	*pūros	*pūro-
millet (cf. 'to grind')	milium	μελίνη	OBr. *mell*	–	–	*m(e)lēi-	*meli	–

Not in Gimbutas PIE forms	*Latin*	*Greek reflexes*	*European reflexes*	*Sanskrit reflexes*	*Buck's reflexes*	*Pokorny's PIE forms*	*Mann's PIE forms*	*Watkins' PIE forms*
ash tree	ornus	-ωνιδος	Ir. *unnius*	*asanah*	–	*ōs-en-	*ōsnos	–
barley (1)	hordeum	κριθη	OHG *gersta*	–	* herzd(h)	* herzd(h)	–	–
barley (2)	farris	παρ-	ON *barr*	*bâr*	–	*bhar-	*bhars-	*bhares
legume/ broad bean	faba	πακός	OCS *bob*	–	–	*bhabhā	*bhabos	*bha-bhā-
flax	lĭnum	λίνον	OE *lĭn*	–	*lno-	*lĭ-no-	*lĭnos	*lĭno-
maple tree	acer	ἄκαρνα	OHG *ahorn*	Ved. *akráh*	–	*aker-	*akeros	–

The same observation applies to other species, most notoriously the oak and the beech, which have been used from the late nineteenth century (e.g. Hehn [1885] 1976; Schrader 1890) right through the end of this century (e.g. Gamkrelidze and Ivanov 1985: 10) as an essential tool for more precisely defining the boundaries of the PIE homeland. Such argumentation may have been rendered obsolete by the lessons from both modern research in socio-linguistics with regard to language dynamics in the present, as well as from the field of historical linguistics concerning the evolution of specific languages within historical times.

For example in Canadian French, 'maize' is called *blé d'Inde*, which translates as 'wheat from India' or 'Indian wheat', because the first French explorers

who arrived in the New World, thinking they had reached the Asian continent, saw that maize was a staple crop for the Native Americans and named it after the staple with which they were most familiar. Similarly, among English-speaking North Americans, *corn* now specifically refers to the maize plant, whereas in Europe it has maintained the generic meaning of 'cereal crop', possibly referring to wheat, barley, rye, or oats, depending on the region. What is noteworthy is that there is a relatively large difference in appearance between, for instance, wheat grass and American maize, and yet the former generated the more popular name for the latter. Therefore, it is perfectly conceivable that a tree rather different from the beech might, for any number of reasons, have been transferred to the beech.

Economy

In Gimbutas' archaeological interpretation of Early Kurgan culture (and, by extension, PIE culture) much weight is given to the Kurgan economy being primarily pastoral in nature, with very little agricultural activity in comparison to neighbouring cultures to the southwest. Gimbutas claims that this lack of agricultural activity is precisely reflected in PIE vocabulary. The preceding subsections on fauna and flora dispute the assumption that the speakers of PIE had domesticated cows, pigs, sheep or horses simply because names for these animals can be reconstructed, and, similarly, admitted that the mere presence of PIE grain-crop terminology gives us insufficient reason to posit an advanced agricultural society. This same point was made long ago by Fraser (1926: 266–7).

However, the reconstruction of terms with the probable meaning of 'butter' and 'wool' (see Table 11.3) suggests that PIE societies had access to *live* animal by-products, or in other words, that they had domesticated (at least to some degree) animals such as sheep, and possibly even cattle. Moreover, we can find equally valid reconstructions for concepts such as 'to plough' and 'to sow'. It seems reasonable, then, to hypothesize that speakers of PIE were familiar with the basic techniques of agriculture and did indeed grow certain crops for their own consumption.

Table 11.3 demonstrates that PIE lexical items referring to agricultural activities are not lacking in comparison with items of a pastoral nature. Thus, one can hardly argue, based on the linguistic data, that Gimbutas' Kurgan economy is unmistakably reflected in PIE.

Technology

Finally, Gimbutas claims that there is solid archaeological evidence for Early Kurgan use of wheeled transportation. This is not disputed; there are various reconstructed PIE forms relating to wheel-technology (Table 11.4). However, there are also equally plausible reconstructions such as **nāu-* and **erə-* which suggest PIE knowledge of navigation, a technology quite untypical of Gimbutas' Kurgan society until after the third wave of migrations.

Furthermore, Gimbutas describes the Early Kurgans as a Copper-Age people, unfamiliar with other metals until the first wave of migration into

Table 11.3 Gimbutas' Kurgan pastoral economy vs PIE agricultural terminology

Pastoral terms	*Latin reflexes*	*Greek reflexes*	*Other European reflexes*	*Sanskrit reflexes*	*Buck's PIE forms (1949)*	*Pokorny's PIE forms (1959)*	*Mann's PIE forms (1984)*	*Watkins' PIE forms (1992)*
to break in a horse	domō	δαμάω	ON temja	*damayāmi*	–	*(demə-), domə-	*domāiō	*demə-
to ride	verēdus	-ριθμός	OE *rīdan*	–	–	*reidh-	*reidh-	*reidh-
to milk	mulgeō	μήλγω	ON *mjólka*	*mṛjámi*	*melĝ-	*mēlĝ-	*melĝ	*melg-
wool	lāna	οὔλνος	ON *ull*	*ūṛṇā*	*wḷnā	*u-ḷnā	*uḷn-	–
to protect, feed	pāscere, pāstor	πατέ-ομαι	OE *fōda*	*pāsyāmi*	–	*pā-	*pāskō	*pā-

Agricultural terms	*Latin reflexes*	*Greek reflexes*	*European reflexes*	*Sk./Toch. reflexes*	*Buck's PIE forms*	*Pokorny's PIE forms*	*Mann's PIE forms*	*Watkins' PIE forms*
field for cultivation	ager	ἀγρός	Goth. *akrs*	*ajra-*	*aĝro-	*aĝ-ro-s	*aĝros	*agro-
grain	–	ζειαί	Lith. *java*	*yava-*	*yewo-	*ieuo-	*ieuos	*yewo-
kernel	granum	–	Ir. *grān*	–	*ĝṝno-	*ĝrē-no-	*ĝṝnóm	*grə-no-
hand mill	gravis	γύρις	ON *kvern*	*gurú-h*	*g^{w}er-	*g^{u}ṝ-nu-	*guernu-	*g^{w}erə-nā
to sow, seed	serere	–	Goth. *saian*	*senā-*	*sē-	*sē(i)-	*sēiō	* sē-
to mow, to reap	metere	ἀμάω	Ir. *meitheal*	–	*mē-	*mē-	*mēi	*mē-
to gather, to pluck	carpere	καπρός	Li. *kerpù*	*kpāṇa*	*(s)ker	*(s)ker-	*kerp	*kerp-
to grind	moloere	Myc. *meretrija*	ON *mola*	*mṛṇámi*	*mel-	*mel-	*mel	*mel-
to grind	frendere	–	OE *grindan*	–	–	*ghren-	*guhrend-h-ō	*ghrendh-
to plough	arare	ἀρόν	Goth. *arjan*	Toch. *āre-*	*ar-	*ar(ə)-	*arō	*arə-

the Balkano-Danubian basin. How applicable this picture is to PIE is subject to debate; lexical roots have in fact been reconstructed (though tentatively) which may refer to gold, silver, and the somewhat vaguer concept of 'ore'.

GIMBUTAS' LINGUISTIC PRESUPPOSITIONS

So far I have suggested, through a comparison of Gimbutas' archaeological data and the lexicon traditionally reconstructed for PIE, that an identification of the Early Kurgan people as the original PIE speakers cannot be empirically validated beyond reasonable doubt in the way she implies. Having thus examined the basic internal problems with Gimbutas' hypothesis, I shall now briefly

Table 11.4 Gimbutas' Kurgan technology vs PIE technology

Kurgan technology	*Latin reflexes*	*Greek reflexes*	*Other European reflexes*	*Sanskrit reflexes*	*Buck's PIE forms (1949)*	*Pokorny's PIE forms (1959)*	*Mann's PIE forms (1984)*	*Watkins' PIE forms (1992)*
axe	ascia	αξίνη	OE *æx*	–	–	*agu(e)s	*aksios	–
sickle	–	–	–	–	–	–	–	–
copper	aes	–	Goth. *aiz*	*àyas*	*ayes-	*aios-	*aios-	*ayes-
wheel	pabillius	κύκλος	OE *hweohl*	*cakra-*	*k^{w}elo-s	*k^{u}ek^{u}lo-	*quequolo-s	*k^{w}(e)-k^{w}l-o
to go or transport in a vehicle	vehere	οχέω	OE *wegan*	*váhāmi*	*weĝh-	*ueĝh-	*ueĝh	*wegh-
wheel hub/ navel	umbilicus	όμφαλός	OE *nafu*	*nábhyam*	–	*enebh-	*nobh-	*nobh-
yoke	iugum	ζυγόν	ON *ok*	*yuga-*	*yugo-m	*iu-go-m	*iugóm	–
cart	–	–	–	–	–	–	–	–
axel	axis	ἄξων	OE *eax*	*ákṣaḥ*	*ak̂s-	*aĝes-, ak̂s	*ak̂sis	–
wagon	–	–	–	–	–	–	–	–
Metallurgy	*Latin reflexes*	*Greek reflexes*	*European reflexes*	*Sanskrit reflexes*	*Buck's PIE forms*	*Pokorny's PIE forms*	*Mann's PIE forms*	*Watkins' PIE forms*
ore	aurum	–	OE *ōra*	*ārah*	–	–	* ros	–
gold	–	τελχίς	Li. geležís	–	–	–	*ghelĝh-	*ghel-
silver	argentum	ἄρψυρος	OIr. *argat*	*rajatám*	–	–	*arĝntom	*arg-
Terms of navigation	*Latin reflexes*	*Greek reflexes*	*European reflexes*	*Sanskrit reflexes*	*Buck's PIE forms*	*Pokorny's PIE forms*	*Mann's PIE forms*	*Watkins' PIE forms*
ship	navis	ναῦς	Ir. *nau*	*nāu-*	*nāu-	*nāu-	* nāu	* nāu -
oar/ to row	remus	ερετμόν	ON *ār*	*aritra-*	*erə-	*erə-	*erət-	*erə-
mast	malus	–	OE *mæst*	–	*mazdos	*mazdo-s	–	–

discuss a few of its *a priori* assumptions concerning the relationship of language and culture, language spread, and linguistic reconstruction.

The question of Early Kurgan linguistic homogeneity

As mentioned above, Gimbutas assumes at the outset that because Early Kurgan-type findings (mostly tombs) share certain material characteristics, they must also reflect a culture which shared one common language (Gimbutas 1975: 298–9). Although such a scenario is certainly possible, it is a speculative one at best, and to accept it without question is a leap of faith which can be challenged by many counter-examples, not only in the modern age, but throughout history.

Hungary is a case in point: surrounded by Indo-European-speaking neighbours on all sides, it has preserved a Uralic speech-community over many centuries.

Culturally, however, it shares many of the material traits which are characteristic of Central-Eastern Europe as a whole. Written records notwithstanding, would archaeologists from Mars assume, 5,000 years from now, that twentieth-century Hungarians spoke the same basic language as their neighbours?

When talking about the Early Kurgan period, we are dealing with a group of *prehistoric* peoples who inhabited a relatively vast area of land, and who left no written records of their language(s). The Eurasian steppe was home to more than one linguistic group and these groups almost certainly borrowed elements from each other's material culture.

Linguistic spread through a conquering elite?

Gimbutas seems to argue that what is lacking in written records can be compensated for by the evidence of tribal migrations; she claims that 'there were no other great expansions and conquests affecting whole territories where earliest historic sources and a cultural continuum prove the existence of Indo-European speakers' (Gimbutas 1970: 156). Here, Gimbutas is referring to the three posited waves of Kurgan migrations, which she uses to account for the dispersal of Indo-European dialects throughout Europe and the Near East.

Indications of cultural spread through time and space may indeed be relevant to the problem of tracing the spread of Indo-European dialects, whether it involved actual population migrations or not. However, Gimbutas' interpretation of the data describes the Kurgan migrations not as a mass movement of populations, but as a series of conquests of neighbouring cultures by a Kurgan warrior elite. According to her scenario, it is presumed that the vanquished populations gradually assimilated the language of their conquerors, in the same way and for the same practical reasons that most historical empires have had a lingua franca.

There are several problems with such a picture. First, it is difficult to imagine how a small elite, powerful as they may have been, could have caused such a dramatic shift in the linguistic make-up of an entire continent. Second, even the use of a lingua franca for economic transactions does not necessarily entail such a language gradually replacing the native tongue of local populations; at best, it becomes a second language spoken only by those whose livelihood depends on it. Third, the mode of diffusion for a lingua franca in prehistory may have been quite different from that of our modern experience.

A foreign language is much more likely to be assimilated by a population if it is taught as part of a standard education and used in various social institutions. These first two conditions apply, for instance, to the role of Latin throughout medieval Europe. I am not referring here to the earlier spread of Latin, which did involve the relocation of Latin-speaking populations and from which the Romance languages subsequently evolved, but to the later use of Latin, even in non-Romance countries, as the language of the Church and of the scholarly community; but as we know, in this context Latin did not come to replace the native languages of its users. Another example

would be the role of French in seventeenth-, eighteenth- and nineteenth-century Europe. Although it was the language of the aristocracy and of diplomacy, the general use of French certainly never spread beyond previously Francophone populations.

Perhaps even more importantly, a foreign language is often able to gain a firm foothold within a local population through its diffusion and popularization by one or more of the mass media, including the written word. Examples of this in a modern-day context might be the use of Swahili in eastern Africa, French in Zaire and other equatorial African countries, or English in post-colonial India. In a prehistoric context, neither one of these socio-linguistic scenarios can be assumed. Conditions favouring the dispersal of a lingua franca (be it PIE or any other language) would have been rare, or even non-existent in the case of written transmission.

The use of PIE as an archaeological tool

Last but certainly not least, the point must be emphasized that PIE is a purely theoretical construct of Comparative Linguistics, and should be treated as such. Though the approach to reconstructing proto-languages was inspired by methods from the pure sciences of the nineteenth century, such as anatomy and biology, the end result cannot be considered in the same scientific light. Pure science involves the independent verification of experiments through a reduplication of their results. Obviously, this has not been achieved by the comparative method in historical linguistics, nor was it to be expected.

The tables on PIE vocabulary items show how the same Indo-European cognates have been used by different scholars to reconstruct significantly different proto-forms. This is a telling indication of the imperfect conditions under which historical (and especially prehistorical) linguists must work, and nevertheless formulate their theories. For both the scholar and the amateur, it is always advisable to keep in mind the inherent limitations of PIE studies.

Concerning the idea of an absolute chronology of PIE, for example, Zimmer (1988) offers the following commentary:

> Every attempt, then to give absolute dates for 'PIE' (or dates for alleged different stages of 'PIE') is either based on the speculative identification of an archaeological culture with the speakers of the 'language of the PIEs' (e.g. Gimbutas, Renfrew, Mallory) or on what may be called 'intelligent guesses', deliberations of probability and feelings of appropriateness (e.g. Meid, Gamkrelidze-Ivanov). The first type of proposal is usually contested by fellow archaeologists and doubted by linguists, the second, being purely subjective because objective arguments simply do not exist, is bound to remain noncommittal. As easily to be seen, many dates of both types have found their way to an often far too sceptical public, and produced strange assertions.
>
> (Zimmer 1988: 372)

In other words, the reconstruction of a PIE lexicon and grammar does not necessarily give us the linguistic picture of a group of PIE speakers at one point in time, or even in one location in space. For one, the information we have on PIE may well represent a linguistic continuum of several millennia into which different lexical items were introduced at different stages. Unfortunately, an internal chronology of the PIE lexicon simply cannot be established with any degree of certainty. This is a point which Zimmer (1990) feels compelled to argue again in a (later) article.

Another problem is the virtual impossibility of *precisely* identifying the original referent of a reconstructed lexical item. Even an approximate reconstruction of its semantic content may be very difficult. It is imperative, in working on the problem of Indo-European origins, that the contents of the PIE lexicon not be treated too literally. Historical linguistics has shown numerous examples of how dramatically the meaning of a given word can shift in the course of a few centuries, let alone several millennia. Any given PIE form may have had different meanings at different points in time within the time-frame posited for the PIE language. Yet Gimbutas does not allow for such an eventuality in her Kurgan–PIE scenario.

CONCLUSION

Given all the internal and external problems with Gimbutas' Kurgan–PIE hypothesis, her arguments should undoubtedly be scrutinized more thoroughly by the linguistic community, the archaeological community, and the popular press who have contributed to making the Kurgan–PIE correlation a widely accepted hypothesis.

On the use of linguistic palaeontology

The use of so-called 'linguistic palaeontology' (i.e. the interpretation of historico-cultural data contained in a reconstructed proto-lexicon) has always been a popular method in the construction of PIE *Urheimat* theories. It rests entirely on the supposition that the *meaning* of a proto-form can be reconstructed beyond a reasonable doubt, a supposition which I argue is false. Furthermore, linguistic palaeontology is a fruitless pursuit in isolation because it depends on the parallel use of other methodologies to arrive at any conclusions. Thus the Kurgan–PIE Hypothesis must have recourse to parallel research, in this case archaeological.

Typically, the end result is one where linguistic palaeontology is conveniently used to corroborate findings arrived at independently, through other methods. Gimbutas seems to establish a Kurgan Hypothesis first, based on purely archaeological observations, and then proceeds to create a picture of the PIE homeland and subsequent dispersal which fits neatly over her archaeological findings. The problem is that in order to do this, she has had to be rather selective in her use of linguistic data, as well as in her interpretation

of that data. This is putting the cart before the horse. Such an unsystematic approach should have given her linguistic proponents real cause for questioning the relevance of her theory, especially if one considers that, by virtue of its nature, the study of PIE is first and foremost a matter for linguistic, not archaeological investigation.

It is unfortunate that linguists (e.g. Gamkrelidze and Ivanov 1985, in seeking corroboration for their typological data) as well as archaeologists (e.g. Mallory 1989) have typically continued to treat linguistic palaeontology as a precarious yet unavoidable step in trying to uncover Indo-European origins. The temptation to fall back on this technique could be due to a general impression that there is no preferable alternative and that it is still the technique most accessible to linguists and non-linguists alike. However other linguistic approaches such as onomastics, i.e. the study of proper names and of river names in particular (e.g. Krahe 1957; Schmid 1978), and typological studies (e.g. Bomhard 1979; Gamkrelidze and Ivanov 1980; Hopper 1982; Diakonov 1985) seem to be yielding rather interesting and potentially less subjective results. Indo-European studies may well benefit from the fine-tuning of these (relatively) newer approaches. The old, pliable crutch of linguistic palaeontology should certainly be abandoned, at least until the theoretical uses and limitations of the PIE lexicon have been more precisely defined.

On the problem of interdisciplinary studies

I have already argued that the problem of Indo-European origins is inherently linguistic and, thus, should be approached primarily through linguistic methods in so far as we can hope to solve it at all. The problem of many homeland theories in the past was that the concept of the 'speech community' was consistently associated with the concept of an 'individual culture', or even with that of a particular 'race'. Though the concern for racial identification has fortunately disappeared from Indo-European studies, the relationship between archaeologically attested material cultures and linguistic groups is still viewed in different ways by different scholars. It is perhaps this lingering ambiguity which has permitted Gimbutas to formulate an elaborate hypothesis on the PIE question, quite independently of the complex theoretical considerations to which historical linguists must attend.

In a 1990 article, the archaeologist Colin Renfrew recognized this pitfall, having participated – alongside other prominent names in Indo-European studies such as Thomas V. Gamkrelidze, Eric P. Hamp, Edgar C. Polomé, Karl Horst Schmidt, John A.C. Greppin, and Thomas L. Markey – in an interdisciplinary conference on *Indo-Europeans and Pre-Indo-Europeans*: 'Certainly I came to see more clearly some of the difficulties in the linguistic field for the alternative thesis which I have been propounding' (Renfrew 1990: 15). Furthermore, he rightly points out that there is a great need for linguists, archaeologists, and anthropologists to collaborate in the development of new models for the dynamics of language dispersal and replacement:

> It is my argument that the use of this kind of reasoning is the only way by which the archaeologist may reach toward the kind of historical reality which could also be intelligible in linguistic terms. Instead, I would go so far as to say that without some such reasoning of this kind, some clear awareness of the kind of archaeological evidence which we may be seeking, there can be no hope of arriving at a coherent view of Indo-European origins.
>
> (Renfrew 1990: 22)

The lack of such a reasoning is the essential crux of Gimbutas' Kurgan–PIE hypothesis. I would like to suggest that the necessary models might be successfully developed through a sufficient number of case studies of language dispersal in historic times. These would have to include as many examples as possible from non-literate speech communities in order to best relate the data back to a prehistoric context.

The problem of the PIE homeland (and that of any other proto-language) is certainly a complex one. To the extent that it can be constructively addressed at all, it is my opinion that its solution may only be sought in a much deeper theoretical understanding between linguists, archaeologists, and scholars from other related disciplines. Through the development of new theoretical models, further research on language change – both at the structural and at the sociolinguistic level – and better interdisciplinary co-operation, the debate over the origins of PIE may perhaps one day be resolved.

NOTE

1 I would like to thank Roger Blench for drawing my attention to this point; his example involved certain African peoples who harvest cereals which grow in the wild, and have evidently harvested them in this way for millennia.

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12 *Archaeology and language in a historical context: the creation of English*

JOHN HINES

INTRODUCTION

In research that combines language, anthropology and archaeology, the lion's share of work is concerned with prehistory. There is a widespread interest in the great language families: the phyla and macrophyla that can be reconstructed on the basis of more or less consistent lexical and phonological correspondences between known languages and whose reconstruction appears to reveal or imply patterns in long-past human population movement as well as providing evidence of the early human environment, both natural and cultural.

The fascination of such topics, however, does not make it at all apparent why there should be so little work on correlating archaeology with language in the historical period generally, let alone in historically well-documented contexts. As the familiar quip about the archaeology of historical periods being either text-aided or text-hindered implies, it can scarcely be the case that archaeologists meekly yield the exclusive rights to reconstruction and explanation to historians as soon as the historical threshold is crossed. As should be obvious, the historical context offers special opportunities for advances in the understanding of the relationship between language and material culture – and thus between linguistic history and archaeology.

The problems involved in carrying reconstructed linguistic histories any great distance back into prehistory are self-defining. When the relevant data are so slender that all have to be used up in making the reconstruction, none will be left to test it. Using data to test a hypothesis is, of course, a means of qualifying and refining that hypothesis, and thus adding those data into it. However, there is a fundamental difference of credibility between this sort of situation, which reigns at the far reaches of reconstructed linguistic groupings, and that of a well-illustrated historical period, where there is a surplus of data, the continued testing of hypotheses against which yields negligible returns in terms of improvement of understanding.

Another critical difference between linguistic prehistory and linguistic history is that the former is based upon a conceptual premise that is rightly

treated as an imperfect model in general linguistics. This is the point-of-origin and diffusionist template that produces the familiar family-tree models of relatively dated linguistic relationships. On the basis of the case study of which a shortened account is given in this chapter, and indeed of other situations, there is good reason to argue that insufficient attention is given to convergent processes which could, for instance, permit the formation of some of the Indo-European sub-phyla or branches very much later than conventional reconstructions consider possible.

It is argued here that the history of English within the North-West Germanic group suggests that a process of assimilation (which in some essential ways is the same as convergence) is an innate tendency of languages in contact. It is also argued that the divergence and divisions between the medieval and modern European languages depend essentially, though not exclusively, upon a particular socio-cultural development and the concomitant politicization of language. In this way an analysis of the origins of English offers certain generalizable propositions and is more than just a particular philological story.

It is not suggested that the incipient politicization or nationalization of language and identity in the European Early Middle Ages referred to here was some absolute evolutionary threshold like the fall of the Tower of Babel, before which all was assimilation and after which all was division and the cultivation of boundaries, either in Europe or worldwide. If we can only genuinely know, by virtue of observation, the rates and types of linguistic changes in given social circumstances in a historical period, we can no more assert what cannot have happened in prehistory in this respect than we can assert what did. I can see no reason to resist the notion that in the vast periods of prehistory, and over the vast geographical ranges of the world's major language phyla, dispersal, isolation (in my view the critical factor) and divergence were dominant trends. It would be preposterous to argue that prehistoric language 'history' must be limited to analogies found in descriptive philologies from the historical period – but conversely the prehistoric branch of archaeolinguistics cannot justifiably ignore the insights of historical philology and modern sociolinguistics. One potentially very serious principle suggested by the following case study can be highlighted to illustrate this point. This is that language as a cultural variable may be more sensitive – or more rapidly sensitive – to group-formation than material culture is. We otherwise have few, if any, subtle formulations of the relationships between language-group and culture-group.

THE GERMANIC MIGRATIONS AND THE ORIGINS OF ENGLISH

It is quite a new departure in both general linguistics and in philology (a term which I use here in the sense of the history of specific languages or language-groups) to explore the possibility of finding significant explanations

of virtually all language change with reference to the historical context of that change. This is an area of theory that goes far beyond relatively simple and disparate historical observations, such as that migration and settlement were required to lay the foundations for some languages, like English or Icelandic, or that contact and interchange were needed to introduce to Germanic peoples such concepts as *wine* or *prostitute* from the Roman Empire in the early centuries AD. It is important to make it clear that the argument is that language change generally needs to be explained with reference to the historical context and not that the historical context reigns supreme in generating, directing and consolidating language change. There is a wide difference between those who emphasize the autonomy of language as a self-governing and highly discrete system – neogrammarians, structuralists and Chomskyans, for instance – and sociolinguists and historians who wish to use linguistic data for their different purposes. It does not, in principle, seem unreasonable to require explanations of linguistic change to make sense in both grammatical and culture-historical terms. The most obvious objection to such a position is that this is easily said but considerably less easy to achieve. The following is a report on some exploratory steps taken in this direction.

For nearly 400 years up to the year 410 AD, much of Britain was within the Roman Empire. The island was partly Romanized. The division between Roman and 'native' spheres was partly geographical, with the most highly Romanized areas to the south and east, a zone beyond these in which the Roman presence was largely confined to military occupation, and a boundary across what is now the north of England leaving an area not incorporated within the Empire to the north. Even in the most Romanized areas there must have been more and less Romanized strata in society – something which is reflected, for instance, by the variety of Roman and traditional building-types that we can find (Millett 1990).

The linguistic situation in Roman Britain is not clear. Both Latin and a Celtic language, Pre-Brythonic, must have existed. The established view until recently was put forward by Kenneth Jackson in the 1950s (Jackson 1953). He noted that Latin loanwords in Brythonic had features typical of formal classical Latin and not of vulgar Latin, and so argued that only classical Latin was used in Britain, for formal purposes by a small elite. This, however, has been disputed, with reference, for instance, to the enormous number of Latin loanwords that there are in the Brythonic language (which becomes Welsh) and on the evidence of a wide range and large quantities of Latin graffiti, from forts, towns and temples (Gratwick 1982). Conversely, it was only in the British Isles, at and beyond the fringe of the Empire, that the formerly widespread Celtic language survived the crushing blow dealt to 'Celticity' by the expansion of the Roman Empire in Europe. It is impossible, as a result, to form any clear picture of what the situation was, but it is reasonable to postulate a diversified state of affairs, with (Pre-)Brythonic and Latin stable in certain substantial domains, and contact leading at least to acrolectal (Latin) influence upon the native Brythonic language.

In the fifth century, the Germanic language and Germanic culture were introduced to Britain in a form that rapidly developed into an English language and culture that were there to stay. The success of Germanicization was rapid. Within about 100 years, Germanic material culture was dominant in the richest, previously most Romanized, areas of Britain; whether or not the Germanic language was equally dominant then we do not know, although a small number of runic inscriptions do give us a glimpse of it being in use (Hines 1990, 1991). These Germanic settlers, or at least their descendants, reorganized their identities, in groups such as Angles, Saxons and Kentishmen that could eventually be linked together in the common category of English. These settlers inherited identities from their homelands, but then redistributed them, so that a century after the settlement inherited names like Angle and Saxon simply do not always refer to the same groups as they would have done before. Of these new English 'peoples', the Anglian group is very hybrid, the Saxon and Kentish groups less so. Archaeologically, however, in no case can we identify any marked native British element in group-formation. The obscure state of Latin–Celtic relations in Roman Britain may be of relevance to the success of the English language. The redistribution of Anglo-Saxon identities can be followed because the settlers apparently made great efforts to define and promote a shared, Germanic/English identity. This may well have been in the context of a native population that was simply confused and divided over its own language and identity, between Roman and British (Hines 1994).

The sociolinguistic theory that I wish to bring to bear in this situation can, I think, be summed up in two basic principles. The first of these is that languages are habitually used by groups and individuals as 'acts of identity':

> the individual creates for himself the patterns of his linguistic behaviour so as to resemble those of the group or groups with which from time to time he wishes to be identified, or so as to be unlike those from whom he wishes to be distinguished.
>
> (Le Page and Tabouret-Keller 1985: 181)

Modern sociolinguistics usually gives examples of this sort of use of language that involve the use or abandonment of contrastive lects (dialects or sociolects) within what can be called one language, rather than the use of what most would recognize as quite different languages. Thus in Ireland the development and spread of distinctive forms of Irish English has provided more material for study than the promotion of the Irish Gaelic language (see e.g. Ureland and Broderick 1991: 559–694). However there is no reason in theory why such huge language contrasts should not be covered by the same basic principle, and the occurrence of Gaelic substrates in some Irish English varieties illustrates how difficult it can be to conceptualize totally separate languages (Harris 1984). It is also observed that lectal differences within a language tend to be marked in the phonology (pronunciation) and vocabulary of the language; much more rarely in morphology (e.g. inflectional endings) and syntax (see Hudson 1980: esp. 43–8).

The second basic principle – a closely related one, in fact – is that language varieties are created and spread by the targeting and imitation of admired and coveted prototypes. BBC English used to be highly influential in this way, unlike, interestingly, what is really the Queen's English – a quite antiquated aristocratic accent. Even more interestingly, Prince Edward, the Queen's youngest son, can be heard adopting a London-based accent that is now spreading very strongly among younger people in Britain (Honey 1989: esp. 79–96; Wales 1994). William Labov's analysis of such speech behaviour among various groups in urban Philadelphia and New York, and in a conservative community on an island, Martha's Vineyard in New York State, are well known (Labov 1972). The same phenomenon is recognized in creole studies, with the relationship between the creole languages originally developed in black slave communities and standard English being represented in the form of a 'post-creole continuum', with targeted attempts to move from a low, creole 'basilect' to the standard 'acrolect' through a series of intermediary 'mesolects', with occasional inversion of the system affording prestige to the basilect – a process called 'recreolization' (Bickerton 1973). Work by James Milroy in particular has done much to illustrate how language change can spread in this way, through people weakly linked to the groups and centres where distinctive language is cultivated – like, perhaps, Prince Edward (Milroy 1992).

But how could we apply these principles to the study of Germanic Europe between a thousand and fifteen hundred years ago? Of course we have very little direct linguistic evidence to look at. We do have runic inscriptions from the later second century AD onwards, but most of these are concentrated in a small area (see most recently Düwel 1994: 56–175). Through comparative philology, however, we can reconstruct languages. I shall concentrate here on reconstructions within the phonological system, because that is the area most amenable to reconstruction and consequently is the most fully studied area. In itself this implies substantial lexical study, because we cannot compare sound-divergences between languages if we cannot postulate common lexical roots. The study in typological terms of historical Germanic syntactical developments is likely to prove very informative, but it is not possible to report on work in that field yet.

Philological reconstruction implies that there once existed a Common Germanic (CGmc) language. The quite large number of Scandinavian runic inscriptions from around AD 150–550 seem to represent this language very consistently. A series of divisive changes must at least have begun in the course of this period, generally sound-changes involving the hypothetical set of Indo-European sounds classified as sonorants: phonetically [j, w, l, r, m and n]. Thus we have 'gemination' involving the semi-vowels [j] and [w], leading eventually to oppositions such as ON (Old Norse) *tryggr* vs OE (Old English) *trēow* (true, faithful), or ON *gløggr* vs OE *glēaw* (bright). The CGmc diphthongs develop in different directions, [au], for instance, giving a long *o* over much of the Continent, long *a* in Frisian, *ea* in OE, and largely remaining as *au* in ON:

hence German *rot*, OE *rēad* and Icelandic *rauðr* (red). Long nasalized vowels arise in certain contexts, and we find divisions between language varieties where long *ã* becomes long *a* and where it becomes long *o*: hence ON *gás* vs OE *gōs* (goose). These changes are not likely on their own to have made varieties of Germanic around the time of the English settlements sound highly different. But if we regard their different histories as representing phonological systems with essentially different structures (the structuralist principle), we can see them as representing major divisions within the whole language system (Krupatkin 1970; Steponavičius 1987; Nielsen 1995).

What appears to be an important piece of evidence is a gold bracteate (a type of pendant) with a runic inscription found at Undley, in Suffolk, East Anglia, England. This inscription includes a rune known as the *ōs* rune (Hines and Odenstedt 1987). This English rune-name, which in fact is the name of a type of god, derives from an earlier **ansuz*, and represents a contextually conditioned development of *a* to long *ã* to long *o*. There is now a question over whether in the Undley inscription this rune represents [ã] or [o:] (Nielsen 1995). The point could be of dialectal significance as Proto-Scandinavian (PrSc) also modified the vowel in the word **ansuz*, to give ON *áss* via [ã:s]-; however, PrSc kept a low open rather than using a mid labialized vowel here, and the *ōs* rune is never attested in Scandinavia. The appearance of the *ōs* rune thus means that a contrastive linguistic variant within Germanic was physically embodied within a writing system. In this way it became part of material culture too.

The Undley inscription is dated to the later fifth century. This is not long after the Germanic settlement of Britain had grown into a substantial process. What is more, the archaeological evidence strongly suggests that this pendant was not made in England but on the Continent, possibly even in the Anglian homeland near Schleswig, just south of Scandinavia. Even if it was made in England, it can have been made there only under direct and immediate influence from that area of southern Scandinavia or northern Germany. It can reasonably be argued, then, that the linguistic opposition this inscription represents can be attributed back to Continental North-West Germanic.

We can therefore be confident that at the time of the settlement of Britain the language of the Germanic settlers was diverging. But all histories of the English language quite rightly tell us that practically none of the dialectal differences within Old English can be attributed to linguistic differences existing before the settlement. The differences between the Anglian, Saxon and Kentish dialects of Old English were not inherited from Continental Angles, Saxons and Jutes, or from any other groups (Nielsen 1985). It must be true, in a way, that a single language variety became dominant in the new linguistic area of England. We can represent this, in terms of the principles introduced above, as a targeting of a single variety, a set of norms, which deleted any inherited differences that had been brought into Britain (Figure 12.1).

It is important to appreciate that this target was not just one superior or powerful variety imported from the Continent but was itself a set of forms and rules that was new because it developed and changed very rapidly and

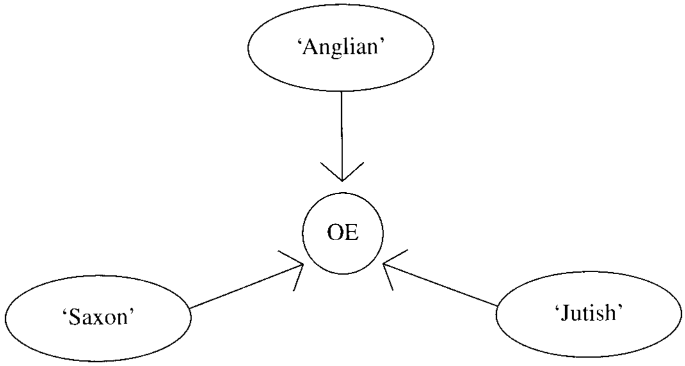

Figure 12.1 The creation of English: model I

extensively from the Germanic of the continent. In a sense, these targeted language shifts had to hit a moving target (Figure 12.2). Of all the Germanic languages, it is English that undergoes the earliest and most extensive innovations in the period after the collapse of the Roman Empire.

But there is another complication. This new English language was never a simple, single language. Within the sound-changes that define Old English, dialectal differences appear almost immediately: i.e. in the reflexes of Gmc *a* and *ǣ* (Campbell 1959: §§127–33). Consequently we ought to remodel English itself synchronically as a sort of solar system, with a central focus and satellite dialects held at a certain distance from it and from one another (Figure 12.3). Each of these in turn could have its own sub-dialects.

In these linguistic models we can see a linguistic parallel to the system of the creation of and relations between different identities and communities within the English system that can be seen in historical and archaeological sources. But there is an important difference in that the archaeological record

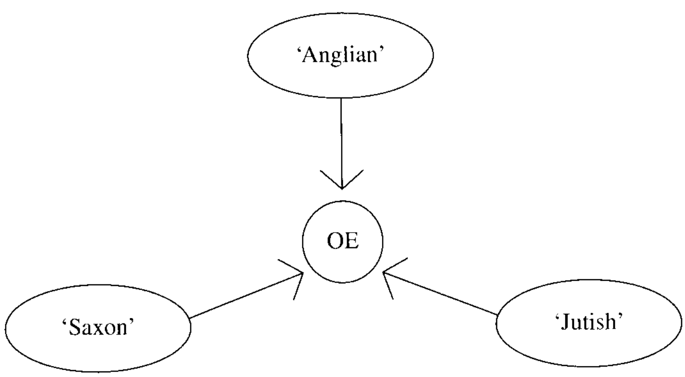

Figure 12.2 The creation of English: model II

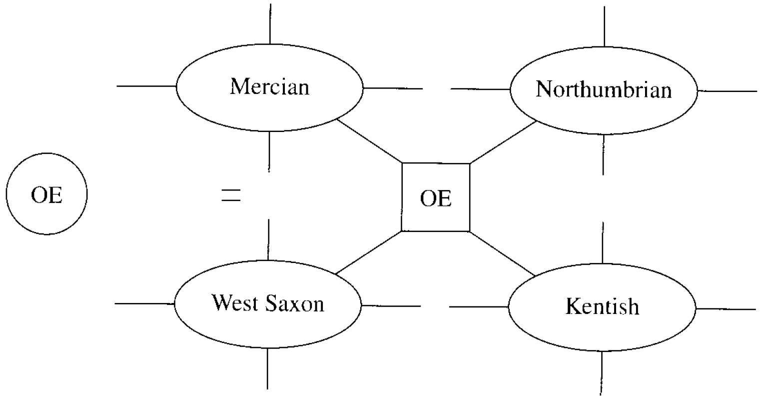

Figure 12.3 The creation of English: model III

indicates a creation of group identities like Anglian, Saxon and Kentish within England first, before the end of the fifth century, while material-cultural indications of a common Englishness for all of these groups appear first about a century later. The linguistic model requires the targeting of a common English set of norms from the very beginning: in chronological terms it would seem simply to be too late to try to postpone the creation of the English language to the late sixth century. The answer may simply be that a common interest and sense of identity that could rapidly be put into effect in language could not be put into effect in ethnic grouping and material culture until considerably later. We do not, of course, know the geographical and social range of the relevant linguistic forms at such an early date. But the extreme rarity of linguistic contrasts attributable to pre-settlement difference in all sources does constitute evidence of some validity for both the early and the thorough unification of English.

Before looking in more detail at relevant aspects of Anglo-Saxon social history, yet another complication must be introduced. This is that in its development after the settlement of Britain, the English language system remained connected to and remained part of a wider Germanic system. After all of the disruptions of the great migrations, we see a North-West Germanic linguistic continuum to be recreated, with historical Old English being most similar to its nearest continental neighbour, Proto-Old Frisian, which in turn is most similar to its nearest neighbour, Old Saxon, which in turn shares distinctive features with Proto-Scandinavian and Old High German, especially the latter, its southern neighbour. What appears to have happened is that Proto-Old English moved dramatically away from the Continental Germanic forms at an early stage. The Continental Germanic languages seem then to have adjusted, to reform the continuum that presumably existed before. Using our principle of targeted language change, we can model this continuum as a chain, with the nodes moving to stay in position (Figure 12.4).

$$\mathrm{OE} \longleftarrow \mathrm{OFr} \longleftarrow \mathrm{OSax}[\leftarrow\text{- - -}\rightarrow \mathrm{ON}]$$

Figure 12.4 The creation of English: model IV

In many respects, this chain is another version of the 'wave model', which sees sound-change spreading, like ripples or waves, from a certain point, fading away over different distances according to the energy of the sound-change. We can see a series of sound-changes apparently beginning first and most strongly in the west, and fading away down this chain: i/j-mutation, which gives us the pairs *food/feed* (OE *fōd/fēdan*), *man/men*; the 'breaking' of monophthongs to diphthongs before certain consonant groups, hence ON *sverð*, German *Schwert*, OE *sweord*; and the palatalization of velar consonants by front vowels, hence Latin *castra* becoming OE *ceastra* and Modern English *chester*.

I/j-mutation, however, is as extensive in Proto-Scandinavian as it is in Old English (although it seems to have occurred there a little later). Despite the contacts that there were between Scandinavia and England at that time, the idea that this sound-change was exported to Scandinavia as an example of linguistic influence is not credible. It is more likely that Old English and Proto-Scandinavian shared inherited structural and prosodic (e.g. tonal) characteristics that eventually led to this mutation being realized to its full extent in these two areas. Then, however, we are left needing to explain what hindered this development in Frisian, Saxon and Old High German, and why the extent of this change should vary in such a regular geographical pattern. The chain model differs from the wave model in that it does not assume one source of innovation and regular patterns of influence. What matters is the totality of linguistic similarity between neighbouring members of the chain, not the absolute regularity of the distribution of sound-changes through it. Innovation can therefore appear anywhere and move anywhere in the chain.

Connections and adjustments like this must depend on contacts across the chain. Archaeologically and historically we can confirm that such contacts existed. The idea that sheer isolation between different groups caused the branching of the Germanic family tree is simply not tenable. Nevertheless divergence between the branches of this family did take place in history, and what is more it seems to take place in the context of ever more regular and intense contacts around the North Sea and down into Germany as the Early Middle Ages progressed (Näsman 1991). Language diversification increases, for instance, while historically a regular network of ports of trade, evidence for long-distance trade in both luxury items (like glassware) and basic commodities (fish and meat), and a plentiful silver coinage, appear between roughly AD 600 and AD 750. A common factor behind all of these material-cultural developments is the consolidation of social aristocracies and royal power within stable political units or kingdoms. The kings established and

protected ports and issued coins; the aristocracy bought and exchanged luxury goods.

The most interesting and far-reaching proposition about language history that this gives rise to is that linguistic flexibility – the ability to adapt, to form a chain – was related to the level of political organization in the communities concerned. With weak political organization, flexibility was high. With stronger political organization, language varieties came under the influence of groups with power whose interest it was to mark their territories by asserting differences, in language as much as in anything else. Thus in modern times, a *Nynorsk*, New Norwegian, had to be created in preparation for a newly independent Norwegian state: a language to be more different from standard Danish than the existing *Riksmål*, 'state-language'. Thus in place of, or even as well as, the important foci and targets for language forms, we now have a deliberate cultivation of linguistic boundaries (for a congruent analysis of the Romance languages in the earlier Middle Ages see Wright 1982, 1991).

I can illustrate this point with an anecdote drawn from an eighth-century historical source, Bede's *Ecclesiastical History of the English People*. Augustine the Roman missionary landed in Kent in 597 with Frankish interpreters whose presence is noted as a necessary practicality and then ignored as of no further significance. From around 660, however, the story can arise of the West Saxon king Cenwealh expelling a Frankish bishop Agilberct – whose very name would have stubbornly failed to show either i-mutation or breaking! – on the pretext that he (Cenwealh) knew only the *lingua* ***Saxonum*** (language *of the Saxons* [my emphasis]) and was *pertaesus barbarae loquellae* (fed up with [the bishop's] barbarous speech) (Bede *HE*: III.7). The adjective *barbarus* is not necessarily a crude insult here. If Frankish were recognizably like a distinctly archaic variety of English, it could indeed have appeared uncouth to a speaker of a prestigiously modernized English. To underline the point, in 664 Bishop Agilberct reportedly used Wilfrid as an English interpreter at the Synod of Whitby (Bede *HE*: III.25). This might, of course, be an isolated and insignificant story. But it is fully consonant with the general theory already derived.

It is in fact apparent that there can be no purely linguistic evidence that will confirm this hypothesis; there are, so far as I know, no linguistic changes that could be explained only in this way. We have instead to turn to metalinguistic evidence: evidence of attitudes to and deliberate uses of language – like the story from Bede just cited. The survival of stories like that is always going to be a random matter. There is, however, another category of linguistic evidence that was made material and has been preserved for us in a systematic way. This is writing. The very writing system any culture adopts – alphabetic, syllabic, pictographic, hieroglyphic – itself reflects something of the perceived structure of the language. Variation between scripts, between Uncials and Insular, futharks and futhorcs, can be used to mark and emphasize perceptions of difference between utterances. Why should the adoption

of a new *ōs* rune by the end of the fifth century not mark the conscious difference of the /o:s/-group from the /ã:s/- or /ans/-group by this time? The point would be particularly striking if the Undley inscription were made as close to a likely boundary between such varieties as in Continental Angeln. But the importance of writing in the present context does not just rely on the graphemic regularization of phonological oppositions. There is recurrent evidence of the political cultivation of distinctive scripts and textual characteristics in Early Medieval Europe, for instance from the first kings of a unified English kingdom in the tenth century (Dumville 1993) to the Cyrillic script fostered by Emperor Boris I of Bulgaria (Browning 1975: 140–84).

CONCLUSION

One particular point at the centre of the interpretation of the diversely illuminated case study presented in this chapter was highlighted by way of introduction to illustrate the wider implications of this analysis. An even more general way of expressing that point is to propose that the transformations of real linguistic history can be just as much a matter of strategic cultural adaptation as any shift in style, social reorganization, economic relationships and so on. Archaeology, anthropology, linguistics and history belong together because all have major, complementary, contributions to make to an understanding of past and present human traditions and behaviour. The full range of this evidence is at present distinctly neglected.

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Part III

LINGUISTIC MODELS IN RECONSTRUCTING SUBSISTENCE SYSTEMS

13 *A conservative look at diffusion involving Mixe-Zoquean languages*

SØREN WICHMANN

ABBREVIATIONS AND CONVENTIONS

pMZ	Proto-Mixe-Zoquean
pM	Proto-Mixean
pZ	Proto-Zoquean
[ ]	A bracketed indication of the language family to which a given language belongs follows its first mentioning
( )	Parenthesized arabic numbers following glosses refer to the list in Campbell and Kaufman (1976). Small Roman numbers refer to Justeson *et al.* (1985); that source does not itself have a number system, so references to page numbers are given. A capital Roman number indexes the item for which I am responsible.

INTRODUCTION

In Wichmann (1995: ch. 9.3) I offer a critique of Campbell and Kaufman (1976), an article which seeks to identify a direction and to fix a historical time-depth for a large number of loanwords in Meso-America. In all cases the donor is said to be Proto-Mixe-Zoquean and the historical setting is seen as a cultural domination exerted by the archaeological Olmec over the rest of Meso-America. These loanwords are treated *en bloc* as an argument for linking the Olmec up to an early form of Mixe-Zoquean. My critique consisted in pointing out, for a number of the individual putative donor items that their attestations in Mixe-Zoquean languages are not such that the assumed time-depth of about 3,500 years can be sustained.

In this chapter I shall expand upon my earlier work and consider the consequences of my findings for a new understanding of the role of Mixe-Zoquean speakers in Meso-American cultural history. When a differentiation of geographic spread and temporal horizons is considered it is possible to offer a more differentiated picture than that of Campbell and Kaufman (1976),

which reduces the findings to a situation where, ultimately, the Olmecs are the donor and Meso-America the recipient. As another new feature of this chapter I shall also consider the loanwords discussed in Justeson *et al.* (1985) and introduce a new one myself (item (I), p. 312).

My method is conservative. I accept a time-depth only for a given word which harmonizes with the linguistic facts available, I am not predisposed to a given direction, and I am sceptical when forms are not sufficiently similar. To give an example: a Chiapas Zoque form *ʔoco* 'papaya' looks somewhat like Xinca *učun*, Nahua *očonih-tli*, and, to a lesser degree, like Ixcatec $t^yu^2\check{c}u_2$ (all 'papaya' or *Carica papaya* – the tree as well as its fruit). All these forms are taken by Campbell and Kaufman (1976: 84) to support their argument concerning the link between the Olmec and speakers of Mixe-Zoquean. From a more conservative point of view these forms only imply that the item 'papaya' has possibly diffused not very long before the arrival of the Spaniards. The fact that none of the forms can be posited for earlier stages of the evolution of the languages involved argues against an early temporal horizon for their diffusion. Now let us look at the phonological forms. Both the Nahua and the Xinca forms have an /n/, which is missing in the Chiapas Zoque form. A preferred shape of Mixe-Zoquean nouns is CVCV (consonant-vowel-consonant-vowel), so it is unlikely that Chiapas Zoque is the donor and more likely that one of the two other languages donated the form. One scenario would be that Nahua was the donor, another would be that Xinca donated the form to Nahua from which it later spread to Chiapas Zoque. My final comment on the forms concerns Ixcatec $t^yu^2\check{c}u^2$. This is simply too different in shape to be acceptable as a possible borrowing. Unless we can account for /t^y/ as a prefix, the form must be discarded.

The example illustrates how I go about in my treatment of the data collected by Campbell and Kaufman (1976). I am not going to provide equally detailed comments on all of their fifty sets of diffused forms. Forms that I do not accept as loanwords (like Ixcatec $t^yu^2\check{c}u^2$) I simply ignore in my presentation. When I reverse the direction of borrowing I do not mention explicitly that my view differs from that of Campbell and Kaufman (1976). This is not necessary since these authors give examples only of loanwords that, according to them, have moved in the one direction from Mixe-Zoquean to other languages. When I posit a different temporal frame or when my MZ reconstructions differ in shape or meaning from those of Campbell and Kaufman (1976), I follow the alternative suggestions already stated in Wichmann (1995; ch. 9.3).

A detailed, item-by-item discussion of Campbell and Kaufman's list follows in the next section. Figure 13.1 shows the hypothetical genetic 'tree' of the Mixe-Zoquean languages, and Figure 13.2 shows their present geographical distribution.

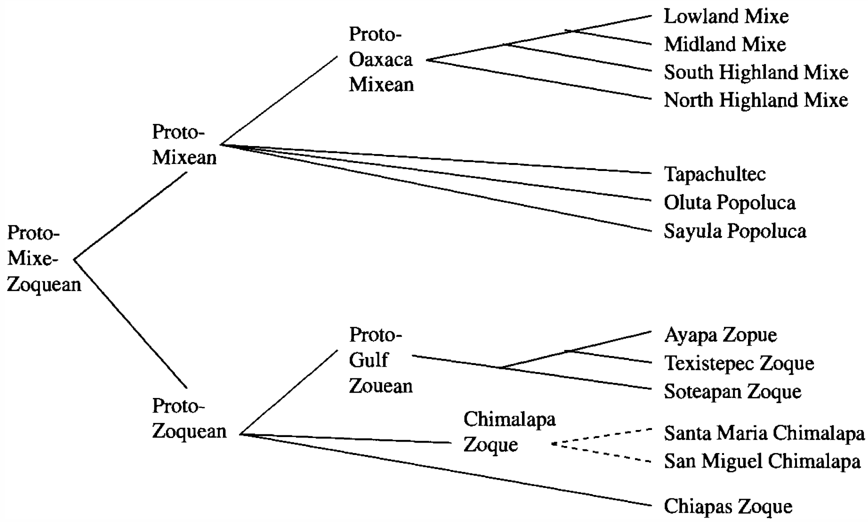

Figure 13.1 Genetic tree of Mixe-Zoquean languages

Figure 13.2 Present-day distribution of Mixe-Zoquean languages with an indication of areas of concentration of some other important languages and language families of Meso-America

THE LINGUISTIC EVIDENCE

(1) CACAO (*Theobroma cacao*). **MZ form**: pAztecan **kakawa* (C/K: pMZ **kakawa*)

Diffused to: pan-Mayan, Nahua, Totonac, Jicaque, Paya, Huave, Lenca, Tarascan, etc.

Comment: Dakin (1995) argues that *kakawa* could be Uto-Aztecan to judge from a morphological mechanism attested in Nahua that could have produced such a form. Her reconstructed Proto-Uto-Aztecan form is **kapa* 'shell'. She argues that the Nahuatl form would be a reduplicated version thereof – reduplication in Nahuatl being sometimes used 'to indicate a smaller or pretend version of an original term of an original term' (Dakin 1995: 1, cf. also Canger 1981) – and finds support for her arguments in forms from various Nahuatl dialects that preserve a meaning 'shell' or 'husk'. Furthermore, she argues that Proto-Uto-Aztecan **kapa* can be broken down into two morphemes **ka* and **pa*. The validity of these last arguments, however, is not a necessary condition for the correctness of the identification of the pan-Meso-American word *kakawa* as a Nahuatl form. I, as others before me, have reconstructed **kakawa* for pMZ, but weakened the hypothesis by pointing to various irregularities in the descendant forms and mentioned 'the possibility that a complicated flow of borrowings between MZ languages, which may or may not have involved non-MZ sources, took place' (Wichmann 1995: 343–4). Most of these irregularities can be resolved if a proto-form having initial stress is posited. But no other trisyllabic forms with initial stress are found in the proto-language, something which makes me no longer willing to consider *kakawa* a native MZ item and instead accept Dakin's hypothesis.

While her hypothesis is attractive on linguistic grounds, the archaeological facts pose some problems. The crux of the matter is that the word *kakaw* was in use among the Maya long before the Nahua are usually assumed to have entered Meso-America. The earliest attestation in Maya hieroglyphic writing is on a vessel found in a grave at Río Azul in Guatemala. The reading of the word is uncontroversial – the word *kakaw* was already deciphered by Lounsbury (1973: 114, 138) albeit in another spelling – and actual cacao remains found within the vessel leave absolutely no doubt that it must be correct. Stuart (1988), who presents the findings, states that the 'pottery in the burial and the style of the painted walls make it clear that this individual [i.e., the one whom the cacao vessel accompanies] lived and died in the Early Classic period, probably in the last half of the 5th century AD'.

For some later attestations it is possible to be more accurate concerning the dating, since the Mayas themselves have provided dates associated with occurrences of the word. Thus, the so-called 'Jauncy Vase' from Buenavista del Cayo, Belize, carries a passage which can be transliterated as yu-ch'V-bi / ta-its-te-le / *ka-ka-wa 'his drinking vessel for the "seasoned" cacao'. Houston *et al.* (1992: 505), who provide this reading, argue that since the

owner or maker of this vessel, nicknamed 'Smoking Squirrel', is mentioned on it, its date probably coincides with the span of his lifetime. His year of birth, recorded elsewhere, is AD 688 and the last date associated with him is AD 719. Another occurrence of the word is on Lintel 3, Piedras Niegras (drawing in Schele 1991: 128). It occurs in a passage which, following broadly accepted readings of individual signs, can be transliterated ti NIGHT u-ch'a-ba ti ka-la ka-ka-wa 'at night they got drunk drinking cacao'. The date associated with the lintel falls in AD 749. Thus the word *kakaw* was current among the Mayas perhaps from as early on as the last half of the fifth century AD and certainly so from the first half of the eighth century onwards. If this is a Uto-Aztecan (i.e. early Nahua) word it follows that Nahua must have been spoken in Meso-America much earlier than what is usually assumed among linguists.

Dakin's etymology hinges directly upon the question of which language was spoken at Teotihuacan, the great metropolis of the Basin of Mexico that exerted its influence in Meso-America in the period AD 300–900. Two candidates have been suggested: Totonacan and Nahua.[1] Among archaeologists the question has been debated hotly and also somewhat in vain – archaeological evidence alone cannot provide a unanimous solution to a question which is linguistic in nature. Among linguists there is a preference for identifying the inhabitants of Teotihuacan as speakers of Totonac rather than Nahua (Campbell 1988). Justeson *et al.* (1985: 26–7), who strive to provide actual evidence, cite four Totonac words that seem to have diffused early into Lowland Mayan (other words are cited but they occur in single languages only and thus cannot be assumed to be early loanwords). The phonological shapes are reasonably similar, but there is quite some divergence in meaning:

Totonacan	Lowland Mayan
'dumb'	'deaf'
'to clear weeds'	'to plant'
'church'	'heart'
'tree'	'*annatto* palm'

These items have a low semantic compatibility and they are not domain-centred, something which would be expected if they had diffused as a result of significant cultural impact (Wichmann 1991: 220; 1995: 219); further, the kind of interaction between the Teotihuacanos and the Mayas is supposed to have taken place between elite groups rather than commoners, something not evident from the words cited here; finally, the list is very short indeed. It seems, then, that evidence from lexical diffusion is not strong enough to build a persuasive argument to the effect that the language of Teotihuacan was Totonacan. So we must look elsewhere for evidence concerning the language of the Teotihuacanos. The most persuasive argument comes from the study of the central Mexican writing tradition, a tradition independent of epi-Olmec and Maya writing and less developed in terms of the degree to which it employed phoneticism. Berlo (1989) identifies a host of close

resemblances between the pictography of Aztec writing and that of mural painting at Teotihuacan and neighbouring sites. In so far as the means are mainly pictographic, one could still argue that the languages of the artists at these two temporal horizons were different. However, the point is driven home by the identification of a set of teeth as a locative marker in what appear to be place names recorded at Cacaxtla (Baddeley 1983: 63) and Xochicalco (Berlo 1989: 28), two sites whose beginnings at AD 700–900 follow immediately upon the demise of Teotihuacan. The observation here is that if this is a rebus style writing, it would only make sense if the language involved were Nahua, where 'tooth' *tlan-tli* is homophonous with *-tlan*, a locative suffix. This particular rebus is also well-known from Aztec pictorial manuscripts (Berdan 1992: 97; Premm 1992: 63).

To sum up, there are internal linguistic arguments for supposing that *kakawa*, a word which has diffused throughout Meso-America, is of an early Nahua origin. If so, it would suggest that Nahua was the language, or at least one of the languages, spoken at Teotihuacan. Having gone this far in accepting the hypothesis we may begin to build up a cultural setting that would account for the diffusion of the word. One obvious difficulty here is that *Theobroma cacao*, while native to Meso-America, does not grow in Central Mexico. The major districts of cacao production now, as in prehistoric times, are regions such as Belize, the Motagua Valley, and Soconusco, adjacent to or within the Maya and epi-Olmec areas (Bergmann 1969; Porter Weaver 1993: 245).

Why would these peoples borrow the word from those to whom they exported its referent? Perhaps it is not so strange after all, precisely because cacao was a difficult-to-obtain luxury commodity among the Teotihuacanos and they could thus have been the ones to perpetuate its importance. It is perhaps significant that the above-mentioned drinking vessel, which, as far as we know, is the first to carry the word *kakaw*, was found deposited together with six pottery vessels of Teotihuacan style (Porter Weaver 1993: 266). Moreover, the site of Río Azul shows cremated burials and cylindrical tripod vessels strongly suggestive of influence ultimately deriving from Teotihuacan (Porter Weaver 1993: 267). While cacao was presumably always known to the Mayas, the Teotihuacanos could have been the ones to turn it into a fashionable luxury commodity; indeed, it is only in the Central Mexican Basin that cacao appears to have gained the status of a means of payment. Thus, Aztec pictorial and early colonial sources tell us that cacao was used as one form of tribute from the provinces to their ruling capital, a custom which could well have been inherited from previous dominant powers in the area.[2]

Conclusion: Most likely diffused from early Nahua.

(2) GOURD[1]. **MZ form**: pMZ **cima*

Diffused to: 'pan-Mayan' (specifically Chol, Aguacatec, Tzutujil, Quiché, Uspantec, Pocomchi, Kekchi: cf. Dienhart 1989), Jicaque.

Conclusion: Diffused from Early Mixe-Zoquean, perhaps even pMZ.

(3) SQUASH. **MZ form**: pM **ciʔwa.*

Diffused to: Huastec, Salvadorean Lenca, Xinca, Tequistlatec, Ixcatec, Chortí.
Conclusion: Diffused in Early Mixean times.

(4) SQUASH, GOURD[2]. **MZ form**: Chimalapa Zoque *ʔawa*

Diffused to: Tequistlatec, perhaps also Miskito, Honduran Lenca, Matagalpa, Cacaopera.
Comment: The Chimalapa Zoque and Tequistlatec forms are identical. *ʔawa* is not attested for any other MZ language.
Conclusion: *ʔawa* most likely diffused from Tequistlatec to Chimalapa Zoque.

(5) TOMATO. **MZ form**: pZ **koya* (not pMZ as C/K would have it)

Diffused to: Chol, Mam, Aguacatec, Teco, Cakchiquel, Tzutujil.
Comment: Justeson *et al.* (1985: 24) partially correct C/K's overstated time-depth by listing Chol *koyaʔ* as a 'late (and perhaps specifically Zoquean)' loan.
Conclusion: Diffused from pZ.

(6) BEAN. **MZ form**: pMZ **sɨk*

Diffused to: Paya.
Conclusion: Diffused from an MZ language.

(7) SWEET POTATO (*Ipomoea batatas*). **MZ form**: pMZ **mɨn(i)* (vs C/K pMZ **mɨnE)*

Diffused to: Totonac *manta*, Jicaque *mɨna*, Xinca *muɨa*, Chinantec *mɨʔ*^{31}, Cuicatec *ʔmiʔ*^{1}*iʔ*^{3}.
Comment: **mɨn(i)* abbreviates pZ **mɨn* as against pM **mɨni*. The latter form is more like the received forms in having two syllables.
Conclusion: Diffused from early Mixean.

(8) PEEL. **MZ form**: pMZ **kɨh* 'peel' (vs C/K pMZ 'edible tuber')

Diffused to: Zapotec, Chatino.
Comment: The meaning 'edible tuber' attested only in Sayula Popoluca and Texistepec Popoluca. The original meaning was something like 'peel' or 'root'. Sayula Popoluca and Texistepec Popoluca together constitute an area of internal diffusion within the MZ language group; it is likely, then, that the specific meaning arose here. Besides, Zapotec *gu* and Chatino *kuu* do not resemble the form very much.
Conclusion: Not diffused from MZ.

(9) ZAPOTE (a variety of trees and their fruits are possibly referred to; they include *Pouteria mammosa*). **MZ form**: pZ **sapane* 'zapote' (vs C/K pMZ **sapani* 'plantain')

Diffused to: Huave *sapɨn* 'zapotillo'.
Comment: All Zoquean languages but Chimalapa Zoque have substituted the meaning 'zapote' for the meaning 'plantain'. Also, both Huave and Chimalapa Zoque are spoken in Oaxaca.
Conclusion: A borrowing from a relatively late form of Zoquean.

(10) GUAVA (a tropical tree, *Psidium guajava*, and its fruit, from which preserves may be produced). **MZ form**: Sierra Popoluca *pátaŋ*

Diffused to: Tzeltal, Tzotzil, and 'several other Mayan languages' (these must be Mopan, Chortí, Chol, Tojolabal, Jacaltec, Motocintlec, and Kekchí: cf. Dienhart 1989).
Comment: Delgaty (1964) cites a Tzotzil form *potow*. This does indeed look like a pre-Zoquean form, since pre-pZ **pataw* would have gone to pZ **pataŋ*.
Conclusion: Diffused from pre-pZ to pan-Mayan.

(11) PAPAYA (*Carica papaya*; according to Schoenhals 1988: 86 '[l]eaves and fruit provide the enzyme used in meat tenderizer which quality was known to the ancients'). **MZ form**: Copainalá Zoque *ʔoco*.

Diffused to: Xinca, Nahua.
Comment: Both Xinca *učun* and Nahua *očonih-tli* have unexplained nasals. Final nasals are uncommon in Zoquean, where CVCV is a preferred pattern, and would likely be dropped if an item was borrowed.
Conclusion: I assume that *ʔoco* was borrowed into Chiapas Zoque (of which Copainalá is a variety) from either Xinca or Nahua.

(12) MARMALADE FRUIT (*Pouteria mammosa*). **MZ form**: pM **kaʔwak* 'marmalade fruit' (vs C/K pMZ **kaʔwak* 'zapote').

Diffused to: Huave, Mixtec.
Conclusion: Must have diffused at a late stage of differentiation from the geographically contiguous Oaxaca Mixean sub-group.

(13) MANIOC (YUCCA) (*Manihot* sp.). MZ form: pMZ **pisi* 'yucca'

Diffused to: Totonac.

(14) DOUGH[1]. **MZ form**: pMZ **waye* 'dough' (derived from pMZ **way* 'to grind') (vs C/K 'pozole' from 'to grind corn')

Diffused to: pMayan **wah* 'tortilla', Totonac *waʔt* 'tamal', Jicaque *we* 'leached corn, tamal', pChiapanec-Mangue **wihʔ* 'tortilla', pChinantecan **wih(n)* 'tortilla'.

Comment: The Oto-Manguean forms have a hard-to-explain different vowel. The pMayan form is also somewhat different from the pMZ one. The best candidate for a recipient of **waye* is the Jicaque form. The next-best is the Totonac form. There are problems with the other forms. pMZ is younger than pMayan and *waye* and *wah* differ both in shape and in meaning: 'dough' is the material, 'tortilla' the product.
Conclusion: Very problematic.

(15) LEACHED CORN. **MZ form**: pMZ **pici* 'leached corn'

Comment: C/K cite forms that all begin *pac* . . . or *paʔs* . . . They all have to do with the maize complex, but none actually means 'leached corn'. While some sort of diffusion is involved, this has nothing to do with pMZ **pici*. C/K also cite North Highland Mixe [Totontepec] *pʊ:ca* 'tamal de Todos Santos'. This may have been borrowed into MZ from an outside source, since final vowels were dropped in Oaxaca Mixean and may form part of the same complex.
Conclusion: Not valid.

(16) PINOLE (a kind of beverage made of ground maize). **MZ form**: pZ **poʔte* 'pinole' from pZ **poʔt* 'to grind corn' (both of which C/K wrongly place in pMZ times)

Diffused to: Nahua *potonki* 'harina muy molida'. Actually the form is *poto:nki* and the full gloss in Molina (1571: 83v) is 'cosa olorosa, o hedionda, o poluos secos y muy molidos, o harina muy cernida' ('smelling or stinking thing, or dry and finely ground powders, or well-ground flour').
Comment: We should be suspicious of this item merely on the grounds that if it diffused to Nahua it could not have been from the Olmec, who were in Meso-America a millennium earlier than the Nahua. As it turns out, even on morphological grounds the item fails as a loanword candidate: *poto:nki* is regularly derived from *poto:ni*, a verb meaning 'to smell bad' by means of the so-called 'participial' suffix (Launey 1979: 110).
Conclusion: Not valid.

(17a) TO GRIND. **MZ form**: pM **hɨc* 'to grind' (vs C/K pMZ **hɨc* 'to grind')

Diffused to: Proto-Mayan **xuč'* 'to grind'
Conclusion: Since the Mayan form is the older one, this must have diffused from Mayan to Mixean.

(17b) DOUGH[2]. **MZ form**: pM **hɨci* 'dough' (linked to pMZ **hɨc* 'to grind' in C/K)

Diffused to: Xinca *huc'i* 'nixtamal, corn dough'.
Comment: Does look like a borrowing in the direction suggested by C/K.

Conclusion: I hypothesize that the verb root went from pMayan to pM, where a noun was derived. This was subsequently borrowed into Xinca.

(18) MAIZE. **MZ form**: None

Comment: This set deals with forms in Tarascan, Xinca, Lenca, Cacaopera, Matagalpa, Sumu, and Proto-Mixtecan which look like Proto-Mayan *_ʔeʔm_. A form pMZ *_mo:k_ is mentioned, but it is stated that 'terms for maize . . . are borrowed throughout Meso-America, through probably not from MZ' (C/K: 85).
Conclusion: It seems likely that this is a case of diffusion from Mayan that does not involve Mixe-Zoquean.

(19) INCENSE. **MZ form**: pMZ *_po:m(o)_ (vs T/K pMZ *_po:mV_)

Diffused to: general-Mayan (except Huastec), Xinca, Totonac, Tepehua, Tequistlatec, Huave.
Comment: In all recipient languages except Tequistlatec the final vowel is lacking. My pMZ *_po:m(o)_ is based on a pZ form with a final /o/ and a pM without. The Tequistlatec form is _-boma_. The best hypothesis seems to be to take account of the presence or absence of a final vowel in the recipient languages and to identify the recipient accordingly.
Conclusion: The item was borrowed from pM into general-Mayan (except Huastec), Xinca, Totonac, Tepehua, and Huave and from a geographically close Zoquean language into Tequistlatec.

(20) TO COUNT, DIVINE. **MZ form**: pMZ *_may_ 'to count'

Diffused to: Kekchí 'twenty, twenty years', Quiché, Cakchiquel 'twenty years of 400 days each', Otomí 'measurement'.
Comment: The donor is more probably *_may_ 'much', a form homophonous with the verb root that C/K cite.
Conclusion: The MZ form may have diffused from an intermediate MZ language to geographically contiguous languages.

(21) DOG. **MZ form**: pM *_ʔuku_ 'dog' (Justeson _et al._ 1985: 23 write *_uka_)

Diffused to: Yucatec *_ʔuk_, as a calendrical day name.
Comment: C/K cite Huastec _ok_ 'fox' and Kanjolabalan _ʔoʔq_, _ʔoq_ 'coyote' as possible cognates for the Yucatec word but state that they nevertheless believe the Yucatec form to be a borrowing. Justeson _et al._ (1985: 24) add the observation that <_oc_> is also the tenth day name in the ritual calendar;[3] they argue, more cautiously than C/K, that this must be a late borrowing happening after the *_k_ > *_č_ shift in pre-Cholan-Tzeltalan and possibly even later.
Conclusion: Probably entered Yucatecan and Cholan from early Mixean.

(22) AXE. **MZ form**: pM **pušˇ* 'to cut with a machete', pM **puš-an* 'axe' (vs C/K pMZ **pus* 'to cut with a knife or axe', **pusan* 'metal (axe?)'

Diffused to: Nahua *pus-teki* 'to cut', *te-pos-(tli)* 'axe, metal', Pokom *pos* 'stone war axe', *ax pos* 'wonder worker', *pus* 'witch', Cakchiquel *pos* 'polished stone'; Quiché *pos*, *pus* 'to sacrifice men by removing their hearts, to cut, polished stone, magic power', Huave *apš* 'to chop with axe', Proto-Central Otomian **bes-na* 'metal, lead', Proto-Popolocan **pos* 'hard stone'.
Comment: Since the form is limited to pMixean I take it that it diffused from Oto-Manguean languages. If the item had diffused in Olmec times it is strange that there should be no trace of it in Zoquean. Cakchiquel and Quiché have *pus-nawal* 'magic power, witch'. The second compound member is a Nahua form. This points to the possibility of Nahua having been a stage in the diffusion of the item.
Conclusion: The item *pos* 'hard stone' probably spread from Proto-Popolocan into pM, Mayan, Otomian, and Nahua. From Nahua it diffused further into Mayan languages as a result of Toltec influence.

(23) WITCH. **MZ form**: pM **naʔaw* 'old man' (vs C/K **naʔwa(y)* 'old man, husband' borrowed into other languages with the meaning 'witch')

Diffused to: Huave *neawɨneay* 'witch', Nahua *nawal* 'witch, transformer, alter ego', Proto-Chiapanec-Mangue **nu-hwa* 'witch'.
Comment: The Proto-Chiapanec-Mangue, as cited by C/K, contains two morphemes. If it is analysable in this language, it must also be native. MZ languages have their own word for 'witch' or 'shaman', namely **cokʔa* or descendants thereof, so if any word did diffuse with that meaning, this would have been a better candidate than a word with a restricted time-depth simply meaning 'old man'. Sayula Popoluca has *naʔway* as against Oluta Popoluca *naʔa'u* and pOM **na"w*. If there was diffusion from an outside source into Mixe-Zoquean, the discrepancy of the Sayula Popoluca form could be explained. In a few Sayula Popoluca borrowings from Zoquean that entered Sayula Popoluca after the rule of final vowel dropping in this language set in we see the addition of a final /y/. So a donor shape such as *naʔwa* would explain the Sayula Popoluca form *naʔway*. The closest match is Proto-Chiapanec-Mangue **nu-hwa* 'witch'.
Conclusion: The item diffused from Chiapanec-Mangue into Huave, Nahua, and Mixean at a time where final vowel-dropping was an active rule in Sayula Popoluca.

(24) WOVEN MAT[1]. **MZ form**: pZ **pata* (vs C/K pMZ)

Diffused to: Nahua *pet(l)a-tl* < pAztecan **pɨta*; pOto-Pamean **pe*.
Conclusion: Diffused from pZ to pAztecan and (perhaps) Oto-Pamean. That direction suggests itself because an inherited initial /p/ would have been lost in pAztecan.

(25) PAPER. **MZ form**: pZ **toto.*

Diffused to: Mixtec *tutuʔ.*
Conclusion: Diffused from a Zoquean language to Mixtec.

(26) TURKEY. **MZ form**: pZ **tuʔnuk*; pM **tu:tuk*

Diffused to: Tzeltal, Tzotzil, Chuj, Jacaltec, Motocintlec *tunuk'/tuluk'*; Tequistlatec *-dulu*, Jicaque *tolo*, Huave *tel*, Zapotec *touʔ*, Nahua *totol-* 'chicken', Paya *totoni-* 'chicken'.
Comment: Michael Coe (pers. comm. to C/K) says that domesticated turkeys appear as late as AD 300 in Meso-America. The item was also diffused internally in Mixe-Zoquean, i.e. from Zoquean to Sayula Popoluca. C/K indicate that the pZ form was loaned into the Mayan languages whereas the pM form 'together with the PZo form' was responsible for the remaining cases. I take it that the pZ form is in fact the relevant one in all cases since it is easy to conceive of /n/ as having been borrowed as /n/, /l/ or nothing, while the shapes in the borrowing languages are more difficult to account for with a /t/ as input.
Conclusion: Both the linguistic and archaeological evidence support a dating around AD 300 for a diffusion from Zoquean to the various languages involved.

(27) BEE, WASP, WASP'S NEST. **MZ form**: Sierra Popoluca *ʔokwoŋ* (vs C/K pMZ **ʔa:kaw*)

Diffused to: Huastec *ʔokow*, Tzeltal, Tzotzil, Tojolabal *ʔáko* (with unexpected first syllable stress).
Comment: C/K posit a pMZ form where in fact the form is just attested in a single Zoquean language. Huastec is just as likely a donor as Sierra Popoluca.
Conclusion: A late diffusion from Huastec.

(28) SANDALS. **MZ form**: pMZ **kɨʔak* 'sandal' (lit. 'limb leather') (vs C/K **keʔak*)

Diffused to: Nahua *kak(-tli)*, Proto-Oto-Manguean ***(h)kwa(h)(n)²*, Proto-Popolocan **ka²*, Proto-Chiapanec-Mangue **hkahʔ*, Varohio *kahkrá*, Cora *kaʔakaí*, Huichol *ka:kái.*
Comment: The hypothesis concerning diffusion can be sustained only if the Oto-Manguean proto-forms are taken to be built on false cognates and if it is taken that the Uto-Aztecan forms (i.e. forms from Varohio, Cora, and Huichol) do not add up to an etymon in Uto-Aztecan. Besides, the Uto-Aztecan forms are not very similar.
Conclusion: Doubtful.

(29) MAGUEY (*Agave* spp.). **MZ form**: pZ **ʔoho* 'maguey' (vs C/K pZ **oH* 'pulque, magey')

Diffused to: Nahua *ok(tli)* 'pulque' (a drink made from maguey).
Conclusion: Diffused from pZ or a later Zoquean language to Nahua.

(30) SALT. **MZ form**: pMZ **ka:na* 'salt'

Diffused to: Huave *kiniɨk*.
Conclusion: Doubtful, as C/K themselves admit.

(31) WOVEN MAT[2]. **MZ form**: pM **toʔki* (vs C/K **toʔkE*)

Diffused to: Huave *tek*, Totonac *š-tiʔkat*.
Comment: Both Huave and Totonac are isolates and there is no diachronic check on the borrowing hypothesis from the point of view of the putative borrowing languages.
Conclusion: Possibly diffused from a Mixean language.

(32) POT. **MZ form**: Sayula Popoluca *šuy*, Chimalapa Zoque *suyuʔ*

Diffused to: Jicaque *soy*, Pipil *šuh-*, Nahua *šok*, Xinca *suh-*, Proto-Otopamean **su*, Proto-Chatino **su*.
Comment: It is speculative to posit a Mixe-Zoquean origin for the Sayula Popoluca and Chimalapa forms. It seems safer to look at them as having an Oto-Manguean origin. The final glide in Jicaque *soy* seems to presuppose a donor form containing such an element, but I take the development of a glide to be independent in Jicaque and Mixe-Zoquean.
Conclusion: Diffused at a time of intermediate differentiation of the Oto-Manguean languages.

(33) TO HEAT SOMETHING. **MZ form**: pMZ **sam*

Diffused to: Western Mayan **saʔm(-et)* 'comal griddle'.
Conclusion: Diffused from MZ at the time of Western Mayan branching off. But problematical because the derivation is not MZ and the verb root is not attested in Mayan.

(34) TRAP. **MZ form**: Chiapas Zoque (Copainalá) *ko-pace*

Diffused to: Western Mayan **pehc'*.
Comment: The Chiapas Zoque form is derived from the descendant of pMZ **pac* 'to squash' and thus not borrowed from Mayan. The Mayan form cannot be borrowed from MZ, since the form is attested only in Chiapas Zoque.
Conclusion: Not valid.

(35) RAT, MOUSE. **MZ form**: pMZ **cu:k* (vs C/K pMZ **cuk* 'mouse')

Diffused to: Chol *cuk*.
Comment: Justeson *et al.* (1985: 24) notice that the word also occurs in Chontal and correct the vowel length in their reconstruction.
Conclusion: Diffused from a language contiguous to Chol.

(36) RIPE. **MZ form**: pM **ca:mʔ* 'ripe' (vs C/K pMZ **ca:m* 'ripe, good')

Diffused to: Tzeltal *cam* 'good', Xinca *c'ama* 'good'.
Comment: There is no meaning 'good' present in the Mixean form.
Conclusion: Not valid.

(37) FOX[1]. **MZ form**: pOM **wa:hš* (vs C/K pM **wa:s* 'fox')

Diffused to: According to C/K 'some Mayan languages have *waʔš*" but only Chol *waš* is attested in Dienhart (1989); also Tarascan *xiwacɨ*.
Conclusion: Diffused to a Mixean language contiguous to Chol and Tarascan and then generally lost from Mixean and only preserved in Oaxaca.

(38) FOX[2]. **MZ form**: C/K pZ **we:tu* 'fox'

Diffused to: Tzotzil, Yucatec, Tojolabal, Jacaltec, Mam **we:t*; Xinca *we:to*; Mixtec <*vidzu*> 'fox (calendric day name)'.
Comment: The form in C/K must be from Sierra Popoluca; it is not pZ. Since Texistepec Popoluca has *pac-ka:ŋ* it cannot even be pGulf Zoquean. It is thus highly likely that this was borrowed into Sierra Popoluca (if this is the language that C/K refer to) from Mayan.
Conclusion: Diffused from Mayan to Sierra Popoluca, Xinca and Mixtec.

(39) FOG. **MZ form**: ChisZ *nas-oʔna* 'fog' (lit: 'earth cloud') (vs C/K pZ **nas-oʔna* 'fog')

Diffused to: Tojolabal *ʔason* 'cloud', Motocintlec *ʔaso:ŋ* 'cloud', Kanjobal, Jacaltec, Chuj *ʔasun* 'cloud'.
Comment: ChisZ *ʔoʔna* 'cloud' is not pZ; there are somewhat similar forms in other Zoquean languages, but they do not match up regularly. It is, however, a strong argument for Zoquean as donor that the form is complex here, whereas it is unanalysable in Mayan.
Conclusion: Diffused from Chiapas Zoque to various Mayan languages.

(40) CHILD. **MZ form**: pZ **ʔune* pM **ʔunak* (another way of rendering this is as pMZ **ʔunV(k)*)

Diffused to: Tzeltal, Tzotzil, Tojolabal, Chuj, Kanjolabal, Jacaltec, Cholti, Mam *une/unin*, Xinca *ʔone* 'child, immature', Otomí *uɛne* 'baby', perhaps also Nahua *kone-tl*.
Comment: Because of the absent final /k/ in the recipient languages, Zoquean must be posited as the candidate for a donor. While Nahua *kone-tl* seems awkward with its initial /k/, this can be accounted for if the derivation pMZ **ko-ʔunV(k)* 'fosterchild' is posited as the donated form.
Conclusion: Diffused from pZ to various Mayan languages and Xinca, Otomí and (perhaps) Nahua.

(41) MUTE. **MZ form**: pMZ **ʔu:ma* 'mute' (vs C/K 'deafmute')

Diffused to: Chol, Tzeltal, Motocintlec *ʔuma.*
Comment: Diffusion at a stage of complete differentiation of Mayan requires a relatively late MZ stage, e.g. pZ or pM.
Conclusion: Diffused from an early Zoquean language spoken contiguously to the Mayan area.

(42) LIZARD. **MZ form**: pMZ **paci*

Diffused to: Cakchiquel, Quiché *(š)paʔč,* Pokom *patiš*, Tequistlatec *-baciʔ.*
Conclusion: Diffused from an MZ stage of some differentiation, either pM or pZ.

(43) ALLIGATOR. **MZ form**: pMZ **ʔuspin* (vs C/K **ʔuspi(n)*)

Diffused to: Totonac *ušpi* Tepehua *húkšpi*, Tarascan *uspi.*
Comment: Chiapas Zoque and Chimalapa Zoque dropped the final /n/, which is why C/K parenthesize the element in their reconstruction. The final /n/ is also lacking from the recipient forms.
Conclusion: Possibly the donor is a Zoquean language intermediate between pZ and the modern stage of differentiation.

(44) RABBIT. **MZ form**: pMZ **koya*

Diffused to: Huastec *koy*, Huave *koy* (and perhaps others).
Conclusion: Diffused from Mixe-Zoquean, most probably at a late stage of differentiation.

(45) ANT. **MZ form**: pZ **hahcuku* (vs C/K pMZ **(hah)cukuʔ*)

Diffused to: Mixtec *tiyókó*, Nahua *cika-(tl)* (from pAztecan **cɨkV* < **cukV*), Huave *čok*, Cacaopera *suku-l.*
Conclusion: Diffused from pZ to Mixtec, Nahua, Huave, and Cacaopera.

(46) OPOSSUM. **MZ form**: pZ **cihi* (vs C/K pZ **ci(ʔ)*)

Diffused to: Xinca *seʔ,* Nahua *siʔ(-tli)* 'hare'.
Comment: C/K adjust the proto-form to the shape it assumes in the borrowing language. Nevertheless, borrowing is a real possibility – at a late pZ (or even later) stage.
Conclusion: Diffused from pZ or a later Zoquean language.

(47) COYOTE. **MZ form**: pZ **pahu* (vs C/K pMZ **pa:huʔ* 'coyote')

Diffused to: Paya *pa:kuʔ*, Mixtec *wáʔū.*
Comment: A pMZ form cannot be posited on the basis of the attestations (Sayula Popoluca *pa:hu*, Chiapas Zoque *pau*, Texistepec Zoque *pahka:ŋ*). This

kind of internal MZ distribution has more than sixty parallels (Wichmann 1995: 213–19), something which I see as as a reason for positing a pZ form and assuming that it was borrowed into Sayula Popoluca.
Conclusion: Diffusion from pZ or later to Paya and Mixtec.

(48) GOURD[3]. **MZ form**: pMZ **pok(ok)* (vs C/K **pok(A)*)

Diffused to: Chol *pok'*, Totonac *po:qoʔtnuʔ*.
Comment: The reconstruction **pok(ok)* is an abbreviation of different reconstructions for pZ and pM, where the former is the monosyllabic one. The Chol form seems to be of Zoquean origin; the Totonac form must surely be morphologically complex, and it is not clear what the root is. Justeson *et al.* (1985: 24) more cautiously list the item as an example of 'a late (and perhaps specifically Zoquean)' loan into Chol.
Conclusion: Diffused from a Zoquean language contiguous to Totonac and Chol.

(49) ELDER BROTHER. **MZ form**: pMZ **ʔaci*

Diffused to: Quichean **ʔac*, Mamean **ʔacik*, Tarascan *ac-i* 'woman's younger brother'.
Comment: Although C/K introduce this set by a 'maybe', borrowing seems likely enough. It must have happened at a relatively early stage for the form to have entered into the intermediate Mayan languages pMamean and pQuichean.
Conclusion: Diffused from pZ or pM to some intermediate Mayan languages and to Tarascan.

(50) IGUANA. **MZ form**: Midland Mixe <*Juun*> 'iguana' (a day name in the ritual calendar)

Diffused to: Mam *ʔoʔn*, Teco *xo:ʔon*, Motocintlec *ʔo:haʔn*, Quiché *ʔoʔon*, Yucatec *huh*, Cholti <*hu*>, Chortí *hu(h)* (in these languages the word retains its meaning but is not used as the ritual day name).
Comment: The set is introduced by 'perhaps' by C/K. The hypothesis that the Mayan forms could have a foreign, specifically Mixean, origin because of the unique sound correspondences involved is reiterated in Justeson *et al.* (1985: 73, n. 14).
Conclusion: If diffused, this could have taken place at the pM stage.

(i) WILD FELINE. **MZ form**: pOM **ʔi:š* 'weasel' (Justeson *et al.* 1985 have 'cacomixtle', a kind of wild feline)

Diffused to: A Mayan day name **(h)iʔ(i)š*, the fourteenth in the ritual calendar.
Comment: The argument of Justeson *et al.* (1985: 24) is that the day name, although it means 'jaguar', is not based on any known Mayan word for jaguar; this could be explained if the word had been borrowed.

(ii) MONKEY. **MZ form**: pMZ **ca:wi* (Justeson *el al.* 1985: 24 reconstruct this as pMZ **cawiʔ*)

Diffused to: Quiché, Chol, Yucatec *čowen/čuwen* (approximate phonetic forms common to these languages), the eleventh day name in the ritual calendar.
Comment: Justeson *et al.* (1985: 24) point out that the addition of a final /n/ is paralleled by a pM form **ciwa* 'squash' (more correctly: **ciʔwa*), which turns up in Chorti as *č'iwan* 'huisquil, chayote' (a kind of edible tuber).

(I) MASTER, OWNER. **MZ form**: pZ **ko-ʔomi*

Diffused to: Yucatecan: Yucatec *yùum* 'lord, owner, god', Itzá *yum-il* 'owner', Mopan *ʔumil* 'master, owner'; Cholan: Chol *yumäl* 'authority, proprietor', Chontal *yum* 'owner' (cf. Dienhart 1989, vol. II: 396–7).
Comment: My pZ reconstruction (Wichmann 1995: 262) builds on pGulf Zoquean **ʔo:mi* and Chiapas Zoque *komi*. The latter is a contraction of *ko-* (a rarely occurring prefix sometimes found on relationship terms) and *ʔomi* 'owner, boss, father, god'. All the Zoquean meanings of the descendants of pZ **ʔomi* recur in Mayan *y-ùm*, *y-um* (*y-* is a lexicalized third person ergative/possessive prefix as is clear from the Mopan form). If the sign numbered T667 in Thompson (1962) can be read as *yum*, the word is attested in the Maya codices, and occurs, among other places, in the epithets of the various manifestations of the rain god Cháak. I propose this reading here as an alternative to earlier, more problematical readings, although proof of the reading in all contexts is still pending.

OVERVIEW OF LEXICAL DIFFUSION INVOLVING MIXE-ZOQUEAN LANGUAGES

Items from pMZ

Gourd[1] (2)	→ Early Mayan

Item from pM or pZ

To count, divine (20)	→ Kekchí, Quiché, Cakchiquel [all Mayan], Otomí [Oto-Manguean]
Lizard (42)	→ Cakchiquel, Quiché, Pokom [Mayan], Tequistlatec [Hokan]
Elder brother (49)	→ some intermediate Mayan languages, Tarascan [isolate]
To heat something (33)	→ Western Mayan

Items from pM or early Mixean

Squash (3)	→ Huastec, pCholan [both Mayan], Lenca, Xinca [isolates], Tequistlatec

Sweet potato (7)	→	Totonac [Totonacan], Jicaque, Xinca [both isolated dialect complexes], Chinantec, Cuicatec [both Oto-Manguean]
Incense (19)	→	General-Mayan (except Huastec), Xinca, Totonac, Tepehua [Totonacan], Huave [isolate]
Iguana (day name) (50)	→	Mam, Teco, Motocintlec, Quiché, Yucatec, Choltí, Chortí [all Mayan]
Dog (21)	→	Yucatecan, Cholan
Wild feline (i)	→	Various Mayan languages
Monkey (ii)	→	Quiché, Chol, Yucatec

Items from pre-pZ or pZ

Guava (10)	→	Early Mayan
Child (40)	→	Various Mayan languages, Xinca, Otomí, Nahua [Uto-Aztecan]
Ant (45)	→	Mixtec [Oto-Manguean], Nahua, Huave, Cacaopera [Misumalpan]
Tomato (5)	→	Early Mayan
Woven mat[1] (24)	→	pAztecan [Uto-Aztecan], Oto-Pamean [Oto-Manguean]
Master, owner (I)	→	Cholan, Yucatecan

Items from early Zoquean

Mute (41)	→	Chol, Tzeltal, Motocintlec [all Mayan]
Alligator (43)	→	Totonac, Tepehua, Tarascan
Opossum (46)	→	Xinca, Nahua
Coyote (47)	→	Paya [Chibchan], Mixtec
Maguey (29)	→	Nahua
Turkey (26)	→	Tzeltal, Tzotzil, Chuj, Jacaltec, Motocintlec [all Mayan], Tequistlatec, Jicaque, Huave, Zapotec [Oto-Manguean], Nahua, Paya

Items from a single Mixean language

Zapote (12)	→	Huave, Mixtec [Oto-Manguean]
Bean (6)	→	Paya
Woven mat[2] (31)	→	Huave, Totonac
Fox[1] (37)	→	Chol

Items from a single Zoquean language

Gourd[3] (48)	→	Totonac, Chol
Incense (19)	→	Tequistlatec
Manioc (yucca) (13)	→	Totonac
Zapote (9)	→	Huave
Fog (39)	→	Various Mayan languages

Items from some MZ languages

Rat, mouse (35)	→ Chol
Rabbit (44)	→ Huastec, Huave

Items diffused from outside of Mixe-Zoquean

Cacao (1)	Early Nahua	→ Rest of Meso-America
Axe (22)	Nahua	→ Mayan
Papaya (11)	Nahua	→ Xinca, Chiapas Zoque
Dough[2] (17b)	pMayan	→ pM → Xinca
Maize (18)	pMayan	→ Tarascan, Xinca, Lenca, Cacaopera, Matagalpa, Sumu [Matagalpan], Proto-Mixtecan
To grind (17a)	Early Mayan	→ Mixean
Fox[2] (38)	Early Mayan	→ Sierra Popoluca, Xinca, Mixtec
Dough[1] (14)	A Mayan language	→ Totonac, Jicaque
Bee, etc. (27)	Huastec	→ Tzeltal, Tzotzil, Tojolabal [all Mayan]
Hard stone (22)	Proto-Popolocan	→ pM, Mayan, Otomian, Nahua; Sierra Popoluca
Witch (23)	Chiapanec-Mangue	→ Huave, Nahua, Mixean
Pot (32)	pOtopamean,	→ Sayula Popoluca, pChatino, Chimalapa Zoque, Jicaque, Xinca, Nahua, Pipil [both Aztecan]
Squash, gourd[2] (4)	Tequistlatec	→ Chimalapa Zoque

THE ARCHAEOLOGICAL EVIDENCE

Three archaeological horizons may be selected for correlations involving Mixe-Zoquean languages. One is that of the Olmec. Some see this as a kind of unitary mother culture of Meso-America (e.g. Coe 1968: 7), while others call into question the idea of a unity and espouse a more regional view (for the range of viewpoints see Sharer and Grove 1989). Olmec remains, such as the well-known colossal heads, are centred around the Gulf Coast of Mexico, but artefacts in Olmec style have a much wider radius of spread and Olmec influence is felt throughout prehistoric eastern Meso-America. The term 'Olmec' has two senses. One is a strict archaeological sense according to which the Olmec are confined geographically to their Gulf Coast heartland and temporally to the phases San Lorenzo (1200–900 BC) and La Venta (900–400 BC), named after two important sites. The other sense allows for everything to be Olmec that is stylistically related to artefacts from the Olmec

heartland. We shall avoid the second sense here, since by using it we would lose the control over chronologies and locations, which are essential in an attempt to build correlations.

The other important horizon is the one called 'Izapan'. Again archaeologists have practised a confusion of senses, mixing precise stratigraphic and more vague, stylistic definitions. In its strict archaeological sense, the Izapan horizon is associated with the archaeological site of Izapa, a ceremonial centre on the Pacific coast of Chiapas near the Guatemalan border. While the site appears to have been inhabited for almost 3,000 years (1500 BC to AD 1200), its major period as a ceremonial centre narrows down to around 300–50 BC (Lowe *et al.* 1982). The more loosely defined stylistic definition of 'Izapan' applies to a great many sites on the Isthmus of Tehuantepec and along the coastal strip of Chiapas and Guatemala (Norman 1976: 287–321; Parsons 1986).

The third horizon is strictly speaking not an archaeological horizon as such. Rather, it is the conjunction of a set of artefacts found at isolated locations in eastern Veracruz and central Chiapas that carry inscriptions in the so-called epi-Olmec script, inscriptions which have been shown (Justeson and Kaufman 1993) to have been written in pZ or pre-pZ4 and which date to the second century AD.[4] Although, from the stylistic point of view, the inscribed monuments and artefacts qualify as 'Izapan' it is best not to draw any conclusions from such stylistic observations since then the geographic picture becomes too undifferentiated, and the possibility for evaluating any arguments concerning correlations of geography, prehistory, and linguistics is hampered by the many references to unprovenanced items that are sprinkled throughout the literature of the more art-historic strand. Both the Olmec heartland, the place where epi-Olmec inscriptions have been found, and the site of Izapa lie within the area where Mixe-Zoquean languages are or were spoken in historic times.[5]

CONCLUSION: CORRELATING LANGUAGE AND ARCHAEOLOGY

Linking pMZ up with the Olmec civilization on the basis of loanwords as done in Campbell and Kaufman (1976) is rather hazardous from a more conservative point of view. When each of the proposed loanwords is carefully examined, only one item out of fifty, 'gourd', may justly be said to approximate the time-depth assumed by Campbell and Kaufman (1976). This means that the linguistic identification of the Olmec as Mixe-Zoquean speakers can be retained only with reservation; it hinges completely upon the viability of projecting Mixe-Zoquean back in time and place along the path of the archaeologically documented cultural continuity from the Olmec to Post-Olmec horizons as defined mainly on stylistic grounds.

When we move to a later stage of differentiation, namely the stage where the two main divisions Proto-Mixean and Proto-Zoquean emerge, we find that a rather massive export of words, especially to Mayan languages, starts

to take place. There are seventeen items at this stage and all but three involve Mayan languages. Cholan is involved in eleven cases. Looking at Cholan specifically is interesting in relation to the comparative epigraphic evidence. Cholan is associated with the earliest Mayan inscriptions, and their writing system, while having many innovative characteristics, must be assumed to ultimately derive from an earlier writing system. The most immediate candidate is a version of epi-Olmec writing.

Since the currently best understood specimens of this writing system were written in (pre-)Proto-Zoquean, we may straightforwardly place speakers of this language in the area where specimens of the writing system have been found, i.e. in central and eastern Veracruz along the Gulf Coast. Long count dates on Stela 1, La Mojarra, and the Tuxtla statuette fall in the middle of the second century AD.

While the Zoque appear to have had the upper hand in the development of writing in Eastern Meso-America, the responsibility for the development of the Meso-American ritual (260 day-) calendar can be attributed with some confidence to the Mixe. Four day names in the ritual 260-day calendar were borrowed from Mixean into Mayan languages (cf. items (21), (50), (i) and (ii) on pp. 306, 312). This alone is enough to suggest that the Mixe were instrumental in developing this calendar. But there are other conjectural pieces of evidence to support this contention.

Interestingly, the only places where the old Meso-American 260-day calendar is still in use are among the Highland Maya (Tedlock 1992) and the Mixe of Oaxaca. The full details of the Mixe use of this calendar were not known until 1991 when Frank Lipp published his description. An important part of that description (Lipp 1991: 62–3) is a list of the numbers '1' though '13' of the ritual calendar as contrasted with the numbers used in ordinary affairs. The list sheds light on the disparity between numerals that can be reconstructed for pM and pZ. Among the numbers '1–13' there are different forms that must be reconstructed for each of these two main divisions, but in Lipp's list both the 'Mixean' and the 'Zoquean' variants occur, the ones known from other Mixean languages being the ordinary numbers and the ones known from Zoquean languages being the ritual numbers. The first explanation that sprung to my mind when I saw this list – an explanation hinted at in Wichmann (1994) – is that the Mixe must have borrowed the ritual numbers from their Zoquean linguistic relatives, and I viewed the phonological disparities between the Zoquean numbers and the Mixean ordinary numbers simply as an *impasse*. I am now prone to think that the ritual 'Zoquean' numbers are in fact inherited and that the 'Mixean' ones are tabu deformations produced at a time when the ritual calendar was created. Possibly the Mixe, like the ancient Maya (Thompson 1950: 131–6, Kelley 1976: 92–6), associated certain deities with each of the numbers '1–13'. In any case, they certainly must have had a strong religious feeling toward the ritual numbers.

The tabu deformation hypothesis explains why the phonological differences between the 'Mixean' and 'Zoquean' numerals are not *lautgesetzlich* [phonetically

regular] although the forms clearly seem to be related. It also resolves the apparent contradiction that the influence on the Maya calendar can be traced philologically to Mixean while Mixean should have had its ritual numbers from Zoquean. The consequences of the hypothesis are far-reaching. One consequence is that we may now allow ourselves to reconstruct the set of pMZ numerals in its entirety rather than cut off its history at the time of the major division as in Wichmann (1995: 524–5). Another consequence, perhaps of more pertinence in the present context, is that we can state with quite some confidence that the Meso-American calendar could be a Mixean invention.

Justeson *et al.* (1984: 148) have previously suggested, albeit without presenting any specific arguments, that an early form of Mixean was spoken at the archaeological site of Izapa and adjacent regions. The possible intersection of two lines of evidence can be brought to bear on this issue. The philological evidence, which we have already discussed, points to an early Mixean origin of the ritual calendar. Another line of evidence points to Izapa as the place where this calendar originated. The idea was brought forward already by Nuttall (1928: 132–3) that the system of reckoning with a 260-day period could have originated along the latitude 14° 42′. At this latitude, she observed, the sun is south of zenith at midday for 260 and north of zenith for 105 days. Malmstrom (1973) takes over the idea (which he fails to credit to others) and adds the observation that Izapa lies on the said latitude and therefore identifies this site with the place of origin of the 260-day calendar (for a full discussion of this and other calendrical features of Izapa see Lowe *et al.* 1982: 275–89).

The intersection of these two lines of evidence results in a linguistic identification of the inhabitants of Izapa as Mixes. I would prefer to present this hypothesis in a strong, narrow form so that it may more readily be revised or refuted. Thus, my hypothesis is that the language spoken at Izapa was simply pM and I would narrow down the period for which I hold this to be true to 300–50 BC This is the period where Izapa appears to have been most vigorous, that is, the period when nearly all plaza groups reached their present proportions and most of its about 250 stone monuments were erected (Lowe *et al.* 1982: 133).

At a later temporal horizon Nahua enters the scene. A significant finding of our investigation is that all Mixe-Zoquean loans in Nahua can be traced specifically to Zoquean. The items are: pAztecan 'woven mat'[1] (from Pre-pZ or pZ), Nahua 'child' (also from from Pre-pZ or pZ), 'opossum', 'maguey', and 'turkey' (from early Zoquean). These items show that during the whole period of dominance by Aztecan speakers Zoquean speakers continued to be a significant factor in Meso-American culture. The earliest loans may have significance with respect to religion ('child') and power ('woven mat') and may have entered during the dawn of Teotihuacan dominance. Later loans are connected with material culture and domestication ('maguey', 'turkey').

There seems to have been an interaction involving both elite and commoners and it seems to have taken place over an extended period. I

would prefer to place the speakers of the Early Zoquean donor relatively close to the Central Mexican area. Their habitat could have been coastal as it appears to have been traditionally, and a correlation involving an archaeological site with a long sequence seems appropriate. One candidate for a specific site that could have been inhabited by Early Zoquean speakers is Cerro de las Mesas in southern Veracruz, described by Porter Weaver (1993: 229) as an important centre of Classic times with roots going back to 600 BC. She goes on to inform us:

> Although its several dozen mounds have been known for a long time, little scientific work has been done at the site. Among the stone monuments, carved stelae with bar-and-dot numerals bear Long Count dates of AD 468 and 533. Some glyphs look derived from Oaxaca, whereas many stone carvings resemble Cotzumalhuapa and Izapa. Cerro de las Mesas seems to mark the northern limits of the Long Count dating system.

The period that I have identified as one in which 'early Zoquean' is spoken can apparently be estimated to begin around 300 AD (cf. the discussion under TURKEY (26) in the introduction) and would have at least until 600 AD, to judge from the Long Count dates.

The contiguity and continuity of Early Zoquean dialects could have been broken as a result of a Totonacan takeover centred around El Tajín in Veracruz. This, then, would the scenario for the break-up into Chiapas Zoque, Chimalapa Zoquean and Gulf Zoquean. The Chimalapas in Oaxaca are currently inhabited by Zoquean speakers who probably emigrated from Veracruz at the time of the supposed break-up. In the course of time these Zoquean commoners experienced some interaction with groups already present in Oaxaca, borrowing a word for 'squash, gourd' from Tequistlatecs and donating their word for 'zapote' to the Huaves.

The hypotheses developed may be summarized as shown in Table 13.1.

The choice of selected focal sites and periods for the correlations is quite deliberate. It seems unreasonable that pM or varieties of Early Mixean should be confined to one place and a mere 250 years. But while it is easy enough to hypothetically extend the correlation to a greater time-span and to include adjacent regions, it would only blur the schematic nature inherent in any correlation of this sort and make it difficult to evaluate and produce arguments for or against the correlation. Another highly schematic feature of my proposal is that I see Zoquean and Mixean influence emanating from the north and the south, the Gulf and the Pacific, respectively. Evidently, this is an idealization, but it is a model which follows directly from our observations and is not apparently contradicted by any other evidence.

Along with Mixe-Zoquean speakers other groups continue to exert their influence in Meso-America. In pMayan times speakers of this language were instrumental in developing the typical Meso-American maize complex, cf. 'dough', 'maize', 'to grind'. Speakers of Oto-Manguean languages seem to

Table 13.1 Dating of stages of reconstructed Mixe-Zoquean

Language	*Focal sites*	*Focal periods*
pre-pMZ?	San Lorenzo	1,200–900 BC
pMZ?	La Venta	900–400 BC
pM	Izapa	300 BC-50 BC
pZ	[inscriptions][6]	AD 100–200
Early Zoquean	Cerro de las Mesas	AD 300–600

have had an early significance with respect to ritual, cf. 'hard stone' and 'witch' (see under axe (22)). The Teotihuacanos appear to have spoken an early form of Nahua. One of their contributions was to raise the value of cacao to a luxury commodity reserved for the elite and donate their name for it to the rest of Meso-America, whence it would eventually spread to the whole world. Finally, when the Aztecs entered the scene they adopted and further perpetuated Meso-American ritual techniques, cf. 'axe'.

I have tried to show that a refinement of the temporal perspective in the study of loanwords may bring about altogether new insights into possible ways of correlating language and archaeology. By taking a conservative look at the directions and datings of Meso-American loanwords I have generated a picture in which the significance of one archaeological horizon, that of the Olmec, shrinks to almost nil and gives way to others, namely those of the epi-Olmec inscriptions and Izapa. Undoubtedly some of the scenarios suggested here for the individual loanwords will have to be revised. I do, however, consider the general methodology followed worth retaining in future studies in the field of Meso-American philology.

NOTES

1 In addition, Kaufman (1973) has suggested Otomian as a candidate, but it is doubtful whether he would still uphold that view.

2 Dakin and I are now collaborating on the expansion of her original draft; in our forthcoming paper we intend to discuss the ideas presented here in more detail.

3 The pan-Meso-American ritual calendar, the Maya version of which is often called the *tzolk'in*, is made up of twenty day names that combine with thirteen number coefficients to yield a cycle of 260 days. Another calendar, in Mayanist terminology the *há'ab* or 'vague year', consists of a 365-day cycle more attuned to measuring the agricultural cycle. These two cycles combine to yield a 18,980-day (52-year) cycle, the so-called 'calendar round'.

4 In fact, the identification of the language of these inscriptions as (pre-)Proto-Zoquean is still only a hypothesis. Justeson and Kaufman (1993: 1709) list some 'epi-Olmec vocabulary specifically associated with the Zoquean branch'. Two items are in fact not limited to Zoquean, i.e. the aspectual suffix *-pa* (see Wichmann 1995: 103) and the verb *kip* 'to fight' (ibid.: 329). Other items are highly speculative readings for which the authors do not present evidence in the remainder of the article: -*ʔi* 'optative', *maw* 'to go away', *paki* 'hard, strong, powerful', *pak-kuy* 'bludgeon',

teʔn 'to stand', *tuki* 'water turtle', and yet others that the authors themselves admit are uncertain. They also (ibid.) present three items, which are said to be 'epi-Olmec vocabulary specifically associated with Eastern (Chiapas) Zoque only'. One is *komi* 'lord', which should be *ʔomi* and which is pan-Zoquean, cf. p. 312. Another is an 'uncertain' item *tusi* 'with hair standing on end'– a strange word to encounter in an inscription (we are not told where it occurs) and an etymological imponderable at that (cf. pMZ *tus* 'to become numb'?, Wichmann 1995: 483). The final one, *siʔi*, 'backside, privates' is the most peculiar of all. It is quite an important piece in Justeson and Kaufman's puzzle since they see it as spelled with one of the more frequent signs, which they read as *si*. The context of the reading *si'i* is something seen by the authors as a description of a penis-piercing ritual, their full reading of which is ko-JUʔTZ-kʉ-wʉ ʔi-RULE-siʔi 'his royal privates began to get pierced on behalf of others' (p. 1706). This reading fails, however, because the straightforward meaning of pZ **siʔi* is 'anus', whereas 'penis' should be **kan* (Wichmann 1995: 439, 345). While the 'private part' reading must be excluded on humanitarian grounds, many others can simply not be shown to be right because they do not occur in multiple contexts. A limited portion of the readings do, however, involve signs that recur in different contexts. One is a sign read ma. This occurs in a hypothetical spelling ma-STAR-tzaʔ *matzaʔ*, 'star', where the middle element is iconographically a star. I find the reading for this sign attractive; the same sign recurs together with another sign, read ha by the authors, unfortunately not supported by other contexts. Nevertheless, it looks like the reading ha-ma for *hama* 'day' is a real possibility, given the controlled calendrical contents of the inscription. If, indeed, the word *hama* occurs in the inscription, it is strong evidence for (pre-)pZ as the language in which it is written. I see this as the best evidence for the linguistic identification of epi-Olmec writing.

The merits of Justeson and Kaufman's 'decipherment' are a sound structural analysis, the productive use of what is known about the contents of Maya inscriptions and sign values in that script, and the identification of inflectional elements such as *-wɨ* 'completive', *-pa* 'incompletive', and (the less convincing) *ʔi-* 'third person'. Some phonetic readings of individual words, such as *hama* 'day, nagual' and *ʔawkiʔmɨ* 'rulership', look promising. Other suggestions are presented without arguments or concrete evidence, so only if a fuller exposition of their work becomes available will a more detailed evaluation become possible.

5 While the classifications they adopt are outdated and misleading, the best summary sources on the historical distribution of Mixe-Zoquean speakers are still Thomas (1974: 15–32) and Foster (1969: 452–6).

6 The La Mojarra stela was found less than 30 kilometres away from Cerro de las Mesas and the Tuxtla Statuette looks like a miniature version of Monument 5, Cerro de las Mesas, so a likely focal site for pZ is Cerro de las Mesas, the same site as that of Early Zoquean.

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14 *Linguistic evidence for the development of yam and palm culture among the Delta Cross peoples of Southeastern Nigeria*

BRUCE CONNELL

ABBREVIATIONS

Ab	Abua
CLC	Central Lower Cross
El	Eleme
FCH	Food Complex Hypothesis
Ib	Ibibio
Ko	Kolo
LC	Lower Cross
Ogb	Ogbia
PBC	Proto-Benue-Congo
PCD	Proto-Central Delta
PDC	Proto-Delta Cross
PLC	Proto-Lower Cross
POg	Proto-Ogoni (or Kegboid)
PUC	Proto-Upper Cross
Ukp	Ukpet

INTRODUCTION

Language and history

In the absence of written histories or archaeological findings, one of the few ways of accessing the past is through linguistic evidence. Simply stated, if for a group of related languages a given lexical item can be reconstructed to the parent language then, barring semantic shift, it is assumed that the term's referent must have existed for the speakers of the parent language. In addition, examination of patterns of lexical borrowing can be revealing with respect to contacts between peoples at an earlier stage in history, particularly concerning cultural influences, while evidence of semantic shift and compounding can also be instructive, especially with respect to the introduction of new concepts or material objects, or a change in the relative importance of existing concepts or objects. Consideration of the reconstructed lexicon of a proto-language, then, can inform us as to the cultural, economic, political, etc., practices of the speakers of the proto-language. This chapter attempts to determine the importance of yam and palm culture in the prehistory of the Delta Cross peoples of Southeastern Nigeria through an examination of the vocabulary associated with these cultural complexes among the present-day speakers

of the Delta Cross languages and how it reconstructs to Proto-Delta Cross. The extent to which terminology associated with yam and palm cultures today can be reconstructed to Proto-Delta Cross may be taken as *prima facie* evidence as to the extent to which these cultural complexes existed among the speakers of PDC. The evidence in this regard suggests that while many, or even most of the plants in question were known to the Delta Cross people, their culture, and perhaps agriculture in general, was not as highly developed as it was to become.

Plant names and lexical reconstruction

Williamson (1970, 1993) has provided exploratory and suggestive work on the role of a number of plant species in the culture and economy of West Africa in prehistoric times. One of the major claims emanating from her work is that of the three food plant complexes that have been identified (Murdock 1959) – indigenous, Southeast Asian, and New World – plant names reconstruct differently according to the complex each is associated with. Names of indigenous plants are said to reconstruct to at least Proto-Benue-Congo, and possibly as far back as Proto-Atlantic-Congo; those of Southeast Asian origin possibly to a more recent period,[1] while those of New World provenance should presumably not be reconstructible to any level, but rather to demonstrate a distribution that disregards linguistic boundaries, and reflects trade routes. This hypothesis is referred to here as the 'Food Complex Hypothesis' (FCH). Reconstruction of the Proto-Delta Cross terminology will provide a partial test of the FCH – 'partial', since Delta Cross is a relatively 'young' language grouping, a sub-branch within Benue-Congo. The hypothesis as formulated here predicts that terms associated with indigenous plants should be reconstructible to Proto-Delta Cross, while those referring to Southeast Asian plants should possibly be reconstructible to the proto-languages of the component groups of Delta Cross, if not to Proto-Delta Cross itself. However, should reconstruction be possible, it may not be a straightforward indication that the speakers of the proto-language were familiar with the present-day referent of a given term, due to the machinations of semantic shift and lexical borrowing. Since the four branches of Delta Cross were already established by the time of the arrival of the Europeans, obviously terms for any food plants introduced by them from the New World would not be reconstructible. The apparent reconstructiblity of such a term would be indicative of a semantic shift of a term from an already existing plant. Evidence presented here regarding the FCH and Proto-Delta Cross is ambiguous, and in the end cannot be said to lend it unequivocal support.

The Delta Cross languages

The linguistic grouping Delta Cross consists of four branches: Upper Cross, Lower Cross, Ogoni (aka Kegboid: Ikoro 1989), and Central Delta (Faraclas 1989; cf. Connell 1995a, 1995b). The group is located in SE Nigeria (see Figure 14.1), occupying most of the Cross River basin (Upper and Lower

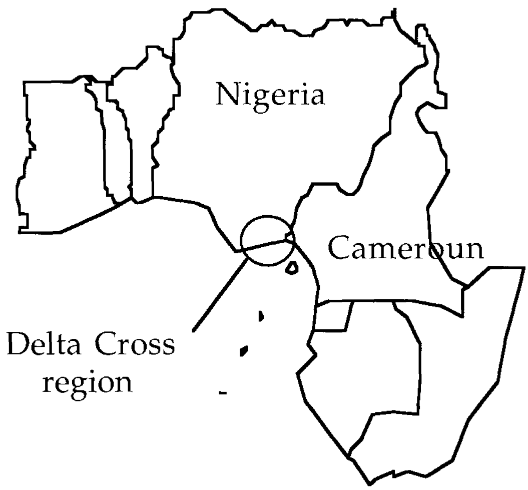

Figure 14.1 Location of the Delta Cross languages within West Africa

Cross), and extending into the eastern half of the Niger Delta (Ogoni and Central Delta). The territory they cover ranges from hilly open forest or savanna to alluvial plain and mangrove swamps. With the exception of a small number of excavations in the Niger Delta (see Alagoa *et al.* 1988), very little archaeological work has been done in the Delta Cross region, and investigation of its prehistory using other sources is also extremely limited (see Connell and Maison 1994 on Lower Cross; Ṣowunmi 1988 reports palynological research in the Niger Delta). With respect to terminology associated with yams and palms, uneven amounts of data are available for the different groups, the most complete coming from Lower Cross, followed by Ogoni, and with the least amount of data currently available for the Upper Cross languages

Yam and palm cultures are known to all of the Delta Cross peoples, but now exist to varying degrees of importance in the different groups. Among the coastal and delta peoples there are those who do not engage in farming to any significant degree, but rather are fishing people who have, for example, fishing festivals rather than yam festivals. This is particularly true of the Agholo (Kolo) and others of Central Delta, and the Obolo of Lower Cross. However, the Upper Cross peoples are farmers for whom the new yam festivals constitute the most important celebration of the year, while the Lower Cross peoples are a mix of farmers and fisherman who in general do not attach great importance to either yam or fishing festivals. Older traditions which may have been associated with, for example, the culture of yams, now may be on the wane. For instance, among the Lower Cross Ibibio, the new yam festival, or harvest festival generally, now involves giving thanks in Church, and is no longer the public village-wide celebration it presumably once was.

YAMS AND YAM CULTURE

Names for tubers

The most important tuber crops used by the various Delta Cross people are the guinea yam (*Dioscorea cayensis*, *D. rotundata*), wateryam (*D. alata*), and 'old' and 'new' cocoyam (*Colocasia esculenta* and *Xanthosoma mafaffa*, respectively). Also used, but to a lesser extent, are the aerial yam (*D. bulbifera*) and the three-leaf yam (*D. dumetorum*). It seems probable that these latter two were formerly used to a greater extent than in the present day (Williamson 1993: Blench 1996). Like the guinea yam they are indigenous to Africa, whereas wateryam and old cocoyam (also called taro in other areas) are of Southeast Asian origin. Burkill (1985, following Coursey 1969) suggests the wateryam was introduced to West Africa by the Portuguese in the sixteenth century, though counter-arguments presented by Blench (n.d.) indicate that *D. alata* has been in West Africa for a much longer period. 'New' cocoyam, referred to as macabo elsewhere, is a relatively recent New World introduction. (The terms 'taro' and 'macabo' are generally unknown in the Delta Cross region, though they are apparently relatively widespread further north in Nigeria and in Cameroon.) There are other tubers referred to as cocoyam which are either indigenous or are varieties of *Colocasia esculenta* which have become nativized. Their botanical taxonomy is unknown, and they are harvested in the wild rather than farmed. Tables 14.1a and 14.1b set out the reconstructed terms for these tubers in the four branches of Delta Cross. The entire dataset is given in the Appendix. When it has not been possible to reconstruct a particular term for a given branch of DC, for example due to lack of data for Upper Cross, representative words are given where possible. (The term 'reconstruction' is used somewhat loosely; in many cases these are not based on systematic comparative work. In most cases, reconstructions are by the author; Proto-Upper Cross reconstructions are from Sterk (n.d.). In many cases, for data not collected by the author, tones were not marked; consequently it has not always been possible to reconstruct tone. Transcriptions have been standardized.)

Summary: names for tubers

It is presumably uncontroversial that speakers of Proto-Delta Cross used yams. A general term for yam (guinea yam), PDC *ɟíɛ́n, that is apparently cognate with one which is widespread outside of Delta Cross, suggests that this term must have existed in PDC. Similarly, the terms *-bɪ̀dɛ̀ and *-dòm must have existed in PDC presumably with the meanings wateryam and aerial yam, respectively. Reconstruction of a term for three-leaf yam is more problematic, although it is generally believed that this is an indigenous plant, in use in ancient times (Blench 1996). However, despite this suggested reconstructibility, what is striking in the Delta Cross data is the apparent switching of referent for the various terms found: semantic shifting has been frequent, such that one term in some cases has referred to guinea yam and in others

Table 14.1a Delta Cross terms for tuber plants

	Yam (gen.)	Wateryam	Aerial yam
PUC	*-tʷɔ̀k; *-dʸén;	–	–
PLC	*-bɛ̀dɛ̀; (*-bíàn; *-díá)	*-bɛ̀dɛ̀; *-ɟá	*-dómò
POg	*ɟíā́	*ya	àdū́ (Eleme)
PCD	*ɛ̀-lɛ̀l	àbìlà (Abua)	ànóm (Abua)
PDC	*-ɟíɛ́n	*-bìdɛ̀; *-ɟa	*-dóm

Discussion

Yam (*Dioscorea cayensis*): Of the four Delta Cross branches, only in Central Delta and Ogoni is it possible to reconstruct single general terms for 'yam' (i.e. guinea yam). These are possibly cognate with each other, though further work is needed to confirm this. The Central Delta root (PCD *ɛ̀lɛ̀l) is the doubtful case; the Ogoni term is more clearly cognate with Upper Cross (PUC *-dʸén), and possibly has outside Delta Cross (e.g. Igbo jí, Engenni ẹ̀ḍịà and Epie àḍịà). Further correspondences between PCD *l, PUC *dʸ and POg *ɟ would be needed to establish the cognacy of terms across the three branches. In Upper Cross, in addition to PUC *-dʸén, Sterk (n.d.) has reconstructed *-tʷɔ̀k; no cognates for this have been found elsewhere in Delta Cross. Other forms are also found in Upper Cross; Sterk has reconstructed *-ggʷòŋ (though the basis for this is not clear), while forms cognate with PLC *-bɛ̀dɛ̀ can also be found, as well as words apparently cognate with forms found for aerial yam (both of these discussed below). Four roots are found in Lower Cross for yam: *-bɛ̀dɛ̀; *-bíàn; *-díá, and úkʷá, none of which is found throughout the group as a general term. Of these, the latter is found only in Obolo. Cognates with it have not been attested elsewhere in Delta Cross, though it is clearly related to Proto-Bantu *-koá. Its origins in Obolo are obscure. The Obolo are a fishing people and do not farm; trade with Bantu-speaking peoples of coastal Cameroon is therefore a possible source for this term in Obolo, though this is not confirmed by examination of terms for yam among the languages in question. The *-díá root is derived from the verb 'eat', and can perhaps be seen as an indicator of the importance of yam as a staple food among the Lower Cross people. Its distribution within Lower Cross suggests that it cannot in fact be reconstructed to PLC, but to a sub-group, Central Lower Cross. Its resemblance to forms such those cited above for Engenni and Epie is probably due to coincidence. Similarly, the form *-bíàn seems reconstructible only to the CLC sub-group, though it could at one time have been more widespread. The oldest root in LC appears to be *-bɛ̀dɛ̀, being the only one of the four for which cognates are found outside Lower Cross. Beyond Central Lower Cross, it refers to guinea yam, while within CLC it means only wateryam (*D. alata*). It turns up only occasionally in Upper Cross (e.g. Ukpet óvènè) where, as it seems to occur only in those languages adjacent to Lower Cross, the possibility of borrowing must be considered. In Central Delta, the term refers to wateryam, as it does in Eleme, the one Ogoni language where it is attested. It is not possible at present, therefore, to reconstruct with confidence one single root as a general term for 'yam' in Proto Delta Cross. The most plausible candidate, though, is PDC *ɟíɛ́n.

Wateryam (*D. alata*): One root, PLC *-bɛ̀dɛ̀, Central Delta (Abua) àbìlà, and Ogoni (Eleme) ebura is found for wateryam. Outside Delta Cross, this root is widespread, stretching across Southern Nigeria, though this is mainly a result of borrowing (Williamson 1993). It is possible that the *-bìdɛ̀ root is reconstructible to PDC; what is less certain is whether it referred to wateryam, although this meaning is tentatively assigned. In some parts of Lower Cross, however, as mentioned above, it means yam in general. In these languages, it also serves for wateryam, though in compound with *-ɟá (e.g. Oro àyábɔ̀rɛ̀), which is a widespread root in Ogoni for wateryam. With the exception of Mbembe, data are not available concerning words for wateryam in Upper Cross, and those found in Mbembe are not cognate with this root.

Aerial yam (D. bulbifera): The data for Delta Cross are incomplete for aerial yam, as forms are not available for Upper Cross, and for Central Delta only from Abua. In Lower Cross and Ogoni a variety of forms exist, conceivably referring to different races, though Ogoni (Eleme) àdũ is cognate with PLC ⋆-dómò. In Central Delta, Abua ànóm is also probably cognate with these, though it also bears a strong resemblance to forms for three-leaf yam, as discussed below. In Upper Cross, forms presumably coming from the same root exist meaning yam generally (e.g. Doko ètùm). Cognates with this root exist as well outside Delta Cross (see Williamson 1993; Blench 1996, who suggest it reconstructs to Proto-Benue-Congo), and therefore PDC ⋆-dòm is reconstructed.

Table 14.1b Delta Cross terms for tuber plants

	Three-leaf yam	*Cocoyam (old)*	*Cocoyam (new)*
PUC	–	ɔkara (Doko)	–
PLC	⋆-nèm; ⋆-gʷeni; ⋆-gʷá	⋆-kpɔ̀ŋ	⋆-nimbo
POg	⋆túú; òtʃũ (Eleme)	⋆-dɛ̀; ⋆-togo	–
PCD	òɗówo (Abua)	⋆-kòlò	–
PDC	–	–	–

Discussion

Three-leaf yam (D. dumetorum): There are three roots which are plausibly reconstructible to PLC, ⋆-nèm; ⋆-gʷeni, and ⋆-gʷá. The first of these is found primarily in CLC, though CD (Abua) ànóm is possibly cognate, if it is not so with forms for aerial yam as discussed above. Lower Cross ⋆ígʷá primarily refers to cassava throughout most of Lower Cross, though it is also used in several LC languages (in compound) to refer to varieties of three-leaf yam, or other wild tubers. A parallel shift has been reported by Blench (1997) for Hausa, where rogo + qualifier is used for various wild tubers, including *D. dumetorum*, while rogo itself refers to cassava. Since cassava is a recent New World introduction (i.e. post-dating the break-up of PLC), it is conceivable that this root originally referred to a type of three-leaf yam and has shifted to cassava following its rapid acceptance as a staple. POg ⋆túú and Abua òɗówò are possibly cognate, though in the absence of more evidence, e.g. in the form of further POg ⋆t – ɗ correspondences, this seems improbable. Neither is cognate with any of the LC forms, while the one form available from Upper Cross (Doko) detriwi, is unlikely to be cognate with any of the other terms found. In summary, despite the three-leaf yam being considered to be an ancient food crop in this region of Africa, and suggestion from Blench (1996) that the PLC root ⋆-nèm reconstructs to PBC, it is not possible to reconstruct a particular root to Proto-Delta Cross.

Old, or 'true', cocoyam (Colocasia esculenta); New Cocoyam (Xanthosoma mafaffa): Williamson (1993) points out that the two types of cocoyam are often referred to by the same common name, with *X. mafaffa* being qualified as the 'European' type. This is true at least in Ibibio among the Lower Cross, where íkpɔ̀ŋ m̀màkárá may be used for *X. mafaffa*, though more commonly the qualifier is omitted. The term therefore can be assumed to have referred to *C. esculenta* before the introduction of *X. mafaffa*.[2] However, two other roots are found in LC, àsìméká and ànìmbò, both apparently referring to *C. esculenta* (i.e. the one that 'scratches the throat' when eaten). None of these words appears to have cognates outside Lower Cross, within Delta Cross, though a similarity between the latter and Tiv ínímbè is noted. The Central Delta form ⋆-kòlò does appear to be cognate with outside forms, as does Doko (Upper Cross) ɔkara, though other available Upper Cross forms do not appear to be cognate. Ogoni shows a number of forms which have been reconstructed as ⋆-dɛ̀; ⋆-togo, neither of which matches up with CD or LC forms, and the first of which may be borrowed from Igbo (cf. édè). However, given that there are a number of races of cocoyam, it is conceivable that the two forms refer to different types of cocoyam, though this has yet to be confirmed. It is not possible, therefore, to reconstruct a single form to Proto-Delta Cross.

to wateryam; in another case, cognates were found referring to guinea yam and others to aerial yam. It is difficult to assess with certainty the implications of this flux. What can be said with most confidence is that tubers were used as food; the switch in referent for certain forms may suggest that a newly introduced (or at least different) tuber gained favour over an earlier preferred one. Hence, in certain Lower Cross languages (e.g. Oro) PDC ⋆-bìdɛ̀ wateryam is used for guinea yam; in Upper Cross (Doko) a term presumably descended from PDC ⋆-dòm, aerial yam, now is used for guinea yam. The possibility remains that PDC ⋆-bìdɛ̀ referred to another tuber and only later shifted to mean wateryam; this is suggested by the compound form found in some LC languages for wateryam, where ⋆-bìdɛ̀ is the base. It seems unlikely that this shift to wateryam would have happened independently in different branches of DC. It is also possible that the Doko form for yam predates other forms, and that rather than having shifted from aerial yam to guinea yam relatively recently, did so in the remote past and has continued to coexist with other forms for yam.

Yam technology and culture

The situation described in the preceding paragraphs reveals only that tuber crops were used by the speakers of Proto-Delta Cross. It is not unambiguously clear which tubers were known at the time and what their relative status and importance was. Moreover, no insight is obtained as to the degree of sophistication and relative importance of yam culture at that time. To gain greater insight into this, it is useful to look at the reconstructibility of terminology associated with the culture of yams. Relevant terminology is presented in Tables 14.2a, 14.2b, 14.2c and 14.2d.

Summary: yams and yam culture

It is apparent that tubers were known at the time of PDC, but the substantial semantic shifting with respect to these terms among the Delta Cross languages since, together with the existence of compound forms for some, suggests that the number of tubers known or used by the speakers of PDC was fewer than subsequently. Blench (1996) points to the importance of accounting for semantic shifts in doing historical reconstruction; the concern in that article is with the shifting of terms for wild plants to those for cultivated plants. Evidence presented in here shows that if any thing this understates the problem, as there has been shifting between terms for cultivated plants and parts of plants, tools used for their cultivation, etc., and not only from wild to cultivated plants. Wilkins (1993) provides an interesting discussion of the importance of understanding semantic shifts in assessing cognacy.

The inability to reconstruct to PDC terms associated with the culture of tubers, such as seed yam, planting end of yam, plant tubers, yam stake, and yam barn suggests that, despite the reconstruction of terms for the various tubers, the technology associated with their cultivation may not have been highly developed among the speakers of PDC. Many, though not all, of the

Table 14.2a Terminology associated with yam culture among the Delta Cross

	Seed yam	*Yam: planting end*	*Stake for yam*
PUC	etum piŋ (Doko)	etum dero (Doko)	berepiŋ (Doko)
PLC	uduot ~	*ú-búkòd ~	*ǹdìsá
POg	*-báà ~	kobee zĩá (Yeghe)	*té ~
PCD	ɛ̀lɛ́l ìbèp	ɛ̀mʊ́ ɛ̀lɛ̀l	òréeɲ ɛ́lɛ̂l
PDC	–	–	–

Discussion

Seed yam: The available data reveal no consistent term for 'seed yam' across Delta Cross, so a PDC term has not been reconstructed. However, there is reasonable consistency within branches of the group. The CD and Ogoni forms are not cognate, but it is noteworthy that at least some languages in both groups render this as a compound of 'plant' (v.) and 'yam'.

Yam: planting end: Similarly, there is no consistent term for the planting end of a yam, though there is some agreement as to the metaphor used; often it is a compound meaning 'head of the yam'.

Stake for yam: Again, for stake, it is not possible to reconstruct a specialized term to PDC. Lower Cross does have a specialized term (used for planting stakes generally), reconstructed as PLC *ǹdìsá; the data available for the other groups indicate that for each a compound of 'yam' and 'stick' is used.

Table 14.2b Terminology associated with yam culture among the Delta Cross

	Planting mound	*Plant (tubers)*	*Dig (uproot tubers)*
PUC	–	*tt^{y}ànɪ́	bò; bu etum (Doko)
PLC	*ebon	*tɔ́ɔ̀	*bòkó; *dɔ̀k
POg	bànà (Nor)	*fo	*bugara
PCD	àɗɪ́βûl ɛ̀lɛ̀l (Abua)	*gbeβ	*buga
PDC	*-bun	–	*bùká

Discussion

Planting mound: On the basis of possible cognates in three of the four Delta Cross branches, PDC *-bon is reconstructed. Only in Lower Cross, however, does the term appear with a high degree of regularity, where it may be related to PLC *-bód, hill. It is not clear whether this can be seen as a specialized term for planting mound in PDC, however a cognate term in Mambila, geographically remote from Delta Cross (e.g. Gembu mbóŋ), lends support to both its reconstrution to PDC and the specialized meaning of planting mound.

Plant (tubers): Generally speaking, there is no specialized term for plant tubers in any branch of Delta Cross; i.e. in each the general word for plant (v.) serves. In Ogoni (Kana), however, kɛb, which may be cognate with PCD *gbeβ, refers specifically to planting tubers, and this latter term, at least in some CD languages, refers to tubers, while in others it includes sow as well. Terms for plant are generally distinguished from sow in the sense of broadcast but not from sowing seeds in a hole. The PCD root may be cognate with forms found in West Benue-Congo, e.g. Yoruba gbìn (see Williamson 1993), where they apparently do not refer, at least specifically, to tubers. With the exception of Upper Cross, where a variety of terms are found, within each branch there is reasonable consistency. There is very little overlap across groups, however, so no term for 'plant tubers' is reconstructible to PDC. Despite this, it is worth pointing out that there are words in Upper Cross which bear a resemblance to the PCD root, e.g. Mbembe (Apiapum) gbóón, and terms found in West Benue Congo, e.g. Isoko kpón.

(Note that the Mbembe term also means 'sow'.) This suggests that this root may be reconstructible to PBC; however, since this form is not widespread in Delta Cross, and since it does occur in other language groups with whom Central Delta and Upper Cross are in contact (e.g. Edoid and Igboid), the possibility of borrowing must first be eliminated.

Dig (uproot tubers): A term does occur across Delta Cross with some consistency, reconstructed to PDC as *bùká. In Lower Cross this is the reversive form of *bʊ̀ʊ́k, bury; if forms across the four branches are cognate, it implies an interesting set of developments (i.e. the replacement of an earlier form for 'bury'), since in other branches of DC the word for 'bury' is not cognate with PLC *bòòk.

Table 14.2c Terminology associated with yam culture among the Delta Cross

	Hoe (v.)	*Hoe (n.)*	*Digging stick*
PUC PLC POg PCD	*kpádɔ̀ ; *ttúnì *dɔ̀k *tɔ̀	*-kʊ̀b(b)ɛ́; *-kɔ̀ɔttí *-dɔ̀k ; *ukide; -kpɔ́kɔ̂p *-tɔ̀; *-kpàbé; *-tùà *ɛ̀sʊ́a	abaere (Doko) *úbè *ɔ̀dɔ̃́ ~ òbúɣáàm (Abua)
PDC	–	*-tùà	*-bè

Discussion

Hoe (v.): A variety of terms exist across the group, with little agreement. LC *dɔ̀k is also a general term for dig (it also occurs in branches of Upper Cross with this meaning); other terms also exist such as Efik (and elsewhere) fúŋ 'turn over the earth', 'cultivate', and Oro tí, 'cultivate'. None of these is readily reconstructed to PLC. Roots containing /kp/ turn up also in Ogoni and Central Delta, but there is no evidence that they are cognate with the PUC term, while POg *tɔ̀ may be cognate with PLC *tɔ́ɔ́, 'plant'.

Hoe (n.): There are several terms in Delta Cross for hoe, but their uncertain cognacy and distribution within the four sub-groups makes reconstruction to PDC tentative. In Lower Cross, most words for hoe are compounds built on PLC *-dɔ̀k, a general word for hoe (and ultimately from PLC *dɔ̀k, dig). Thus in Ibibio, ákpɔ́kɔ̂p údɔ̀k (also called údɔ́k m̀bíòd, literally, hoe + grass) is a curved weeding hoe used by women, while ásák ú!dɔ́k is a large straight hoe used for planting. A different hoe, ákpɔ́kɔ̂p úbè, is used for cocoyam. (Note that for Ibibio, úbê also refers to a long digging stick used for some types of yam, as well as meaning hoe handle.) In some Lower Cross languages, the general term for hoe is reconstructed as PLC *ùkídé, which means axe in other LC languages. Given that PUC *-kɔ̀ɔttí is likely cognate with PLC *ùkídé, hoe rather than axe is probably its earlier meaning. The Ibibio word ákpɔ́kɔ̂p is a compound incorporating kɔ̂p, one meaning of which is to be hooked, curved; PUC *-kʊ̀b[b]ɛ́ is a probable cognate with the LC root -kɔ̂p; indeed given the range of forms on which the PUC reconstruction is based, it is conceivably originally a compound of roots that are separate in LC: -kɔ̂p and *úbè. It is not clear whether POg *-kpàbé is cognate with LC ákpɔ́kɔ̂p; if the POg term is also originally a compound, incorporating the -kpɔ́ and *-be roots, then there is a good case for cognacy here. For Central Delta and Ogoni, Eleme ɛ̀tʃwà provides a link establishing PCD *ɛ̀-sʊ́a and POg *tùà as cognates; however no cognates with this appear in either LC or UC. It is not clear, therefore, which of these roots can be reconstructed to PDC; here, *-tùà only is proposed, without specifying its precise meaning, while others of those discussed should ultimately prove reconstructible. It should also be pointed out that shifts in referent have clearly occurred – cf. axe and hoe in Lower Cross and Upper Cross, and digging stick, handle of hoe, and hoe.

Digging stick: The LC form úbè has possible cognates in Central Delta and Upper Cross, assuming Abua òbúɣáàm and Doko abaere are compounds (èrè in Doko is stick, tree). While,

as mentioned above, this term has other, related, referents in LC, digging stick is quite plausibly the earlier meaning, later shifting to handle of hoe. An identical root úbè, meaning short digging stick, is attested in Igbo, and so the possibility of borrowing cannot be ruled out. Consequently this term is only tentatively proposed as being reconstructed as PDC ⋆-bè.

Table 14.2d Terminology associated with yam culture among the Delta Cross

	Lot of five yams	*Yam barn*	*New yam festival*
PUC	etum bɔruŋ (Doko)	daban (Doko)	etum sabi dutʃa (Doko)
PLC	⋆í-síó ~	⋆é-kò; ⋆í-sè	⋆n′-súúk; ⋆ú-!sɔ́dɔ́
POg	òʔòò íí zĩá (Kana)	⋆ò-gúm / ⋆dá	–
PCD	òòɣ-òòɣ ə̀rìsì íḷɛ̂l (Abua)	è-kùè ı̣̄lɛ̀l (Abua)	ɛ̀yáál ípùlə̀ ìḷɛ̀l (Abua)
PDC	–	–	–

Discussion

Lot of five yams: Yams in the Delta Cross region are typically sold at market in lots of five. There is, however, no specialist term for this that can be reconstructed to PDC. In languages in both Lower Cross and Central Delta there exists a term which translates literally as face of yam, although other terms, and sometimes simply a term five yams, also exist. In Ogoni, and for the data available from Upper Cross, it is this latter which is most common. The similarity between LC and CD might be taken to suggest that this term should be reconstructed to PDC; however given that by and large present-day Central Delta people do not engage in farming (and therefore marketing) of yams, it is more likely the term came to them by way of trade.

Yam barn: Again, here, there is a similarity between CD and LC, with Abua èkùè cognate with PLC ⋆ékò. However, other terms also exist in both groups, and for none of these are there cognates in either of Ogoni or Upper Cross. The form given for UC (Doko) (with cognates elsewhere in UC) is conceivably cognate with Igbo ɔbā, but again borrowing cannot be ruled out. Without greater evidence, therefore, it is not possible at present to reconstruct a term for yam barn in PDC.

New yam festival: Not all Delta Cross peoples place great emphasis on a new yam festival, even among those who are agriculturists. The tradition is strongest (if measured by the degree of public festivities) among the Upper Cross. Among the Lower Cross, the tradition has become syncretized with modern Christian traditions. PLC ⋆ú!sɔ́dɔ́ is a general term for feast, ⋆ú!sɔ́dɔ́ ídɔ̀k is the harvest feast. It is possible that PLC ⋆ńsúúk refers to the more traditional celebration. Cognates across the four branches of Delta Cross have not been found.

relevant terms, however, are reconstructible within the individual branches of Delta Cross. This evidence suggests that the period of the break-up of the Proto-Delta Cross community may have been associated with an increase in development of and familiarity with yams and their culture (cf. Connell and Maison 1994 on Lower Cross).

PALMS AND THEIR CULTURE

Terms associated with oil palm and raphia palm

There are a number of varieties of palms which have varying degrees of importance among the Delta Cross peoples. Among the most important are the oil palm (*Elaeis guineensis*) and the raphia or wine palm (*Raphia hookeri*). The oil palm is used primarily for its fruit, from which oil is obtained (both from the flesh of the fruit and the kernel), though it is also exploited in other ways, the sap for wine, for example, and the fronds in many ceremonies or festivals. The raphia palm has a variety of uses – most importantly its branches are used in house construction, its fibres for weaving different artefacts, and its sap for wine and gin production. Terms for these trees, their parts, and associated artefacts and technology are presented in Tables 14.3a–14.3g.

Summary: palms and their culture

Reconstructing basic terms associated with oil palm and raphia palm to Proto-Delta Cross (and indeed beyond) is relatively straightforward – more so than for any of the yams discussed in the previous section. With terms for the trees themselves, as well as for frond, palm fruit, palm oil, palm kernel, broom, wine and possibly pressing trough being reconstructible to PDC, it should be uncontroversial that both of these trees were known and exploited by the speakers of Proto-Delta Cross. Three points must be raised, however, to qualify this claim; first, there are still several objects or concepts basic to the palm culture, e.g. tap wine, raphia fibre, and palm fruit cluster, for which, at least with our present knowledge, PDC terms are not reconstructible. Second, the fact that a term such as pressing trough is apparently not specific to the manufacture of palm oil means that as a reconstruction in PDC it presents only weak evidence for this technology among the PDC. Third, again there is evidence for semantic shifting and compounding, and as was suggested above in the discussion of yam culture these processes reflect the introduction of new concepts, technology and/or material objects and are therefore plausibly interpreted as evidence of a period of flux or development with respect to the culture of palms among the Delta Cross.

THE ANTIQUITY OF AGRICULTURE IN SOUTHEASTERN NIGERIA

The ready conclusion from the foregoing discussion of yam and palm culture among the present-day Delta Cross people, and the extent to which terminology associated with these cultures can be reconstructed to the latest common ancestor of the Delta Cross languages, is that these cultures were not then developed to the same degree of sophistication as has since been achieved. Since yam and palm culture arguably represent the most important

Table 14.3a Terms for oil palm, raphia palm, associated products and culture

	Oil palm	*Young oil palm*	*Mature oil palm*
PUC	*-ddí; *-tat(t)á	–	–
PLC	*é-ɟòb / a-	*ú-tén	*ókóón
POg	*-ɟoo	*angana ɟoo	*-té ɟoo
PCD	*-ɗè	ɔ̀rìr (Kolo, Abua)	àtèn mááɗè (Kolo)
PDC	*-dde; *-ɟòb; *-ten	–	–

Discussion

Oil palm: Two roots exist as general terms for oil palm, one found in Upper Cross and Central Delta and the other in Lower Cross and Ogoni, and both of which apparently must be reconstructible to Delta Cross. Williamson (1993) reports a cognate with the first root in Ogoni (Kana), though this did not appear in my own data. The former, PDC *-dde, is cognate with forms found in much of Benue-Congo and beyond (cf. Williamson 1993), and is presumably the older of the two. The Lower Cross/Ogoni form, reconstructed as PDC *-ɟòb, is also attested in Upper Cross, but meaning palm fruit or kernel, and outside Delta Cross in Bendi (e.g. Bokyi, Bekwara bù-jɛ̂p). The second form attested as a general term in UC, *-tat(t)á, is in all probability cognate with PLC *ú-tén young oil palm; POg *-té, and CD àtèn, mature oil palm. The variation between young and mature oil palm here gives evidence of semantic shift – further evidence for such shifting exists within LC, where Usaghade útén is a general term for oil palm. Assuming the more widespread root, *-dde, is indeed the older term, two hypotheses can be proposed to account for the occurrence of the second root: either a secondary race of *E. guineensis* became favoured by the Lower Cross and Ogoni, as well as the Bendi or there has been a semantic shift, likely from palm fruit or kernel to the whole tree. This could have occurred as the tree (i.e. its fruit) grew in economic importance. Finally, PDC *-ten is also reconstructed; it is not clear what the precise sense of the term would have been, whether young, mature, or a general term, but we note the similarity this root has with terms found throughout Niger-Congo (e.g. Bambara (Mande) nten, Gã (Kwa) tɛ̀ŋ), Mambila (Bantoid) ter). It is therefore probable that this root is in fact the oldest and goes back to Proto-Mande-Congo (i.e. Niger-Congo excluding Kordofanian). Indeed, it is possible that both PDC *-dde and PDC *-ten are reflexes of the ancient PNC root.

Table 14.3b Terms for oil palm, raphia palm, associated products and culture

	Oil palm frond	*Fibre of frond*	*Leaves of frond*
PUC	iboi (Doko)	iboi (Doko)	itoŋ eyuŋ (Doko)
PLC	*-bàk ~; *n'làk ~	*-fááŋ	*ńlàk ~
POg	*-la -ɟoo	–	lâ (Gokana)
PCD	ògólògo ~ (Kolo)	–	–
PDC	*-làk	–	–

Discussion

Oil palm frond, fibre, and leaves of frond: Reflexes of the PLC root *-bàk primarily refer to the frond stripped of its leaves; reflexes of PLC *ńlàk may refer to the frond with its leaves, or simply to the leaves themselves. The POg root, and possibly the CD form given (assuming ògó-lòg-o), appear cognate with PLC *ńlàk, and hence tentatively can be reconstructed to PDC as *-làk. While it is not clear whether this should be considered to have had the meaning frond or leaves of frond, the former is assumed. The evidence available does not permit the reconstruction to PDC of a term meaning fibre of palm frond.

Table 14.3c Terms for oil palm, raphia palm, associated products and culture

	Palm fruit cluster	*Stem of cluster*	*Single fruit*
PUC	derheruwen (Doko)	deraŋ (Doko)	derheyup (Doko)
PLC	⋆-fèn; ⋆-tò	–	(ŋ̀kùà) áyòp (Ibibio)
POg	⋆-kum	⋆-kpik	⋆-bee; ⋆-kpo
PCD	ègún ~ (Kolo)	ègùm ~ (Kolo)	èkpó ə̀lê (Abua)
PDC		–	⋆-ɟòb

Discussion

Palm fruit cluster: POg ⋆-kum and CD (Kolo) ègún are possibly cognates, though Kolo ègùm 'stem of cluster' is equally plausible, implying a semantic shift from 'stem' to 'cluster'. There are no obvious cognates with Lower and Upper Cross, though further research into phonological change within Delta Cross might establish PLC ⋆-fèn as a cognate with the Ogoni/CD forms.[3]

Stem of cluster: There are no obvious cognates across Delta Cross, though Ogoni roots having a labial-velar in C_1 position may ultimately prove cognate with LC /kp/ forms. LC, however, shows a variety of forms and it is not possible to reconstruct any single one.

Single fruit: Again, there are cognates in both Ogoni and Central Delta (POg ⋆-kpo), with no corresponding forms in LC or UC. LC shows a variety of forms, but it is interesting to note -ɟòb is used either independently or in compound (seed + ⋆-ɟòb) in many languages (corresponding to usage in UC (Doko)). In Efik (and other Lower Cross languages), éyòp is polysemous, referring to both the tree and the fruit. This is suggestive of a process of semantic shift (in this case oil palm fruit to oil palm tree) involving not only a period of polysemy (Wilkins 1993), but also a stage involving compounding. Note also (below) that POg ⋆-bee is used in some languages of the Ogoni group for raphia fruit, and that it corresponds, in each Ogoni language, to the word for head.

Table 14.3d Terms for oil palm, raphia palm, associated products and culture

	Red palm oil	*Oil pressing trough*	*Palm kernel*
PUC	⋆-ntànà; ⋆-nɔ̀ɔ̀mì	ekpuha (Doko)	eyep (Doko)
PLC	⋆á-dàn	⋆á-kúá	⋆í-kíb
POg	⋆-nɔ́ɔ ~	⋆-kua	⋆-ʔib
PCD	⋆àmʊ́nʊ́m	ɔ̀kpɔ̀n (Kolo)	⋆-zib
PDC	⋆-nɔɔm; ⋆-tàn	⋆-kua	⋆-kib

Discussion

Red palm oil: PUC ⋆-ntànà and PLC ⋆á-dàn are likely cognates, while PUC ⋆-nɔ̀ɔ̀mì, POg ⋆-nɔ́ɔ, and PCD ⋆àmʊ́nʊ́m are also assumed to be cognate. The evidence suggests that, although it is not clear what their difference in meaning would have been, both terms can be tentatively reconstructed to PDC as ⋆-tàn and ⋆-nɔɔm, respectively. Further evidence of their wider distribution within DC is desirable to support this.

Oil pressing trough: Forms in at least three of the four DC branches are cognate; CD forms having a velar or labial-velar may also be cognate. PDC ⋆-kua is therefore reconstructed, though evidence from LC, where the root has wider reference than oil pressing trough, suggests that this may not have been a specialized term in PDC.

Palm kernel: Forms from all but Upper Cross are obvious cognates, while the UC form is clearly part of the semantic field (cf. oil palm and palm fruit, above). PDC ⋆ -kib is reconstructed.

Table 14.3e Terms for oil palm, raphia palm, associated products and culture

	Broom from frond	*Tap wine*	*Palm wine tapper*
PUC	ituehe (Doko)	pɔ utɔ (Doko)	opi-utɔ (Doko)
PLC	⋆á-ɟàŋ	⋆tùàk; ⋆kpì	⋆-tùàk; ⋆-kpì
POg	a-sã	⋆ari ~	⋆ne ~
PCD	ɔ̀zànì (Abua)	gbo ~ (Kugbo)	–
PDC	⋆ -ɟàŋ	–	–

Discussion

Broom from frond: The most commonly used type of broom in the DC area is made from ribs of palm fronds; terms from all but UC are probable cognates; PDC ⋆ -ɟàŋ is reconstructed.

Tap wine: There is apparently no specialist term among DC languages for tap wine. The LC terms generally mean hit or strike, the Og terms may mean collect, while the CD term may also mean dig.

Palm wine tapper: Similarly there is no specialized term for tapper reconstructible to PDC. Lower Cross terms are derivative of the verb tap wine, while Ogoni uses a compound incorporating person with the verb tap wine.

Table 14.3f Terms for oil palm, raphia palm, associated products and culture

	Oil palm wine	*Raphia palm wine*	*Raphia palm*
PUC	⋆-tùkkì	–	–
PLC	⋆-mín	⋆ḿ-mín úkɔ̀d; ⋆ú-kɔ̀d	⋆ ú-kɔ̀d
POg	⋆m-mii ɟoo	⋆m-mii ŋkɔl	⋆ŋ-kɔl
PCD	⋆-mim ~	⋆-mim ~	⋆ɔ̀-ɣɔ́l
PDC	⋆-mim ~	⋆-mim ~	⋆ -kɔd

Discussion

Oil palm wine, raphia palm wine: Both the oil palm and the raphia palm are exploited for wine in the Delta Cross region. The term for both is usually the same, reconstructed as PDC ⋆-mim, qualified by the name of the palm in Ogoni and Central Delta. This is a widespread root, occurring outside of Delta Cross. In some Lower Cross languages, the term for raphia wine has become synonymous with raphia palm, i.e. PLC ⋆-kɔ̀d; in a sub-set of these it is generalized to wine, and qualified to indicate wine from the oil palm, e.g. Ibibio, Nkari úkɔ̀d éyòp.

Raphia palm: This term is reconstructed to PDC as ⋆-kɔd. It is cognate with forms found outside DC and appears reconstructible possibly to Proto-Benue-Congo. Williamson (1993) argues it goes back to Proto-Atlantic-Congo, on the basis of its occurrence in Ijoid, but this claim is weakened by indications that similar forms found in neighbouring Yoruboid and Edoid languages are borrowings. If these truly are borrowings, then the Ijoid forms may be so as well; the only occurrences of this root which clearly are not borrowings apparently occur in Eastern Benue-Congo. (See Blench 1993 on EBC; in Igboid, two roots are found, ŋgwọ̀ and ọ̀gọ̀lọ̀, the latter presumably borrowed.)

Table 14.3g Terms for oil palm, raphia palm, associated products and culture

	Raphia frond	*Raphia fibre*	*Raphia fruit/kernel*
PUC	–	–	–
PLC	*ń-làk ú-kɔ̀d	*ń-lááṁ	*-kib ~; ùbê; aya
POg	*-la ŋkɔl	kúrè (Kana)	-ʔib (Kana); beè (Tai)
PCD	–	–	òbógùsì
PDC	*-làk	–	*-kib; *-be

Discussion

Raphia frond: The evidence available shows the same term used for oil palm frond was also used for the raphia palm; i.e. PDC *-làk, had the meaning of 'palm frond' and was qualified to specify oil or raphia palm. At least in Lower Cross, a separate root, -kɔ́ɔ̀k, refers to the raphia frond stripped of its leaves, often referred to as bamboo in English, though -bàk is also used (cf. oil palm frond above).

Raphia fibre: Raphia fibre is widely used in the Delta Cross region, but is not, on the evidence available, reconstructible to PDC. In Ogoni, Kana kúrè and Tai ákwérè are probable cognates, and the Tai form indicates borrowing from Igbo, akwalà.

Raphia fruit/kernel: Across branches of DC there is a mixing of roots for the two glosses, again perhaps suggestive of semantic shifting. PDC *-kib and *-be are reconstructed; note *-kib also applies to palm fruit, and may have had its meaning generalized from this. If so, it is possible that *-be is the original PDC root for raphia kernel.

aspects of agriculture for the Delta Cross people, this conclusion can be perhaps extended to suggest that agriculture in general in this region was in a relatively young stage at the time of the break-up and dispersal of the Delta Cross. The evidence suggests that palm culture was more highly developed than that associated with yams, though we note that even until recent times, the oil palm has been collectively 'owned' and harvested wild. As such, it may not constitute an appropriate judge as to agricultural development.

To seek further insight into the more general development of agriculture, we examine the reconstructibility of three further terms, bitterleaf (*Vernonia amygdalina)*, fluted pumpkin *(Telfairia occidentalis)* and pumpkin leaf, plants of which the leaves apparently were only exploited in cultivated form (Blench, pers. comm.). Though the data for Delta Cross are not complete, available terms are presented in Table 14.4.

Examination of these three terms provides supporting evidence for the notion expressed above, that agriculture may have been at a nascent stage during the time of Proto-Delta Cross. None of these can be reconstructed to PDC; in addition there is evidence of borrowing, suggestive of the provenance of knowledge of the use of these plants with respect to the Delta Cross peoples. This evidence suggests that, for at least some Central Delta and most of Ogoni, knowledge of both bitterleaf, and perhaps to a lesser extent fluted pumpkin, comes from the Igbo, presumably through trade, and that its introduction post-dated the break-up of Proto-Central Delta and Proto-Ogoni.

Table 14.4 Terms for bitterleaf, fluted pumpkin and pumpkin leaf

	Bitterleaf	*Fluted pumpkin*	*Pumpkin leaf*
PUC	otidot (Doko)	ekumidipum (Doko)	ekumidiyuŋ (Doko)
PLC	* -tílòd	*ù-bɔ́ɔ̀ŋ	* -kɔ̀ŋ ~ ; * -fàŋ ~
POg	* -lobiri	*-eeb	–
PCD	àdìrì (Ko); òlúgbò (Ogb.)	éé!βé (Abua)	–
PDC	–	–	–

Discussion
Bitterleaf (Vernonia amygdalina, V. colorata): While it is not impossible that roots in Lower Cross, Ogoni, and Central Delta are ultimately cognate and reconstructible to PDC, this cannot be proposed with confidence. The Proto-Lower Cross reconstruction, *-tílòd, has as its base form *-lòd (cf. Ib. dòdó, become bitter). Obolo has different terms in different dialects, a situation which could profitably be further investigated. One root, ǹtìló, is a reflex of the PLC root; the second is cognate with Ogoni *-lobiri, and the third, àfílàmà is also found in Ịjọ. This latter, however, may ultimately prove to be cognate with the Ogoni term and a reflex of a root which is widespread; cf. Tem (Central Gur) aloma, Berom (Platoid) etulop, and also Dera (Chadic) wólóm. If so, it will provide interesting insight into the early use of bitterleaf; patterns of borrowing of this root between Chadic and Niger-Congo languages need investigation. The LC term is also used in Upper Cross (Doko) and has presumably been borrowed into that language from LC. The occurrence of the Ogoni form in LC is likely a result of borrowing. Og -lobiri is possibly cognate with Igbo òlúgbù, and may be a result of borrowing (cf. Williamson 1970), but doubt is cast on this by the fact that the Eleme term, òlígbò, clearly is cognate with and presumably borrowed from Igbo. In any event, it does not appear that this root can be legitimately reconstructed to POg. In Central Delta there are at least three different terms in use: E. Ogbia òlúgbò is borrowed from Igbo, Abua and Odual ɔ́klɔ̀ has no obvious cognates outside CD, while another CD root, Kolo -ɗírí, exists which may ultimately prove to be cognate with the LC root, though it is not now apparently related to a root meaning bitter, as in LC. It is probably borrowed from Ịjọ, where an identical root exists meaning leaf, medicine. One difficulty in reconstructing a term for bitterleaf in general is that the plant has several uses (leaves as a source of food, medicine, twigs and roots as chewing sticks, etc.), and names are often associated with one or other of these uses (presumably whichever is most important or first known to the people in question).

Fluted pumpkin (Telfairia occidentalis), pumpkin leaf: There is no root for fluted pumpkin which is readily reconstructible to PDC, although Abua (CD) éé!βé is cognate with POg *-eeb. Other roots occurring in these two branches resemble Igbo ʊgʊ̄ and are apparently borrowings. Roots found in Lower Cross (PLC *ù-bɔ́ɔ̀ŋ) and Upper Cross are not cognate with these or with each other, although the PLC root does appear to be cognate with forms found outside Cross River, such as Proto-Manenguba *bog, and Kainji bu-gbɔŋgo. In general, there is no specific term for pumpkin leaf, as it is usually a compound of a general term for leaf and the term for fluted pumpkin. Since *Telfairia occidentalis* itself was presumably harvested in its wild form before the advent of agriculture (as it is today among forest peoples), it was expected that a term for it should be reconstructible to PDC. As it appears not to have been part of the lexicon of Proto-Delta Cross, the plant was presumably not used by, and hence possibly not known to the speakers of PDC; this may ultimately give insight into the location of the PDC homeland, i.e. somewhere outside the natural habitat of *Telfairia occidentalis*.

With respect to the Lower Cross, a term for bitterleaf evolved from the resources of their own language (i.e. PLC), suggesting that trade may not have played a large role in its introduction to the Lower Cross. A similar story can plausibly be told for fluted pumpkin.

CONCLUSION

Generally speaking, the evidence presented in this chapter suggests a perhaps later introduction and development of agriculture in the southeastern region of Nigeria (and perhaps West Africa generally) than has been envisaged in, for example, Williamson (1970, 1993). In making this claim, however, two points should be emphasized. First, the evidence and discussion have dealt explicitly with one (linguistic) group, Delta Cross. It should not necessarily be assumed that agriculture developed at a uniform time and rate, even for peoples who were in reasonably close geographical proximity. There is obviously a range of factors (micro-climate, local vegetation, and density of population among them) which have not been examined in this chapter, but which need to be in order to have a fuller understanding of the development of agriculture among a particular people and within a particular locality. Consequently, specific conclusions reached in this chapter concerning the history of Delta Cross cannot be generalized to neighbouring, let alone more distant, groups. Each, ultimately, should be studied in its own right. Second, no explicit attempt has been made in this chapter to place this development within a specific time-frame. Connell and Maison (1994) have argued that the initial break-up of Proto-Lower Cross must have occurred at least 1000 BP; the degree of divergence among the four branches of Delta Cross (cf. Connell 1995b) suggests a considerable time-period must have elapsed between the break-up of Proto-Delta Cross and that of PLC. Ṣowumni (1988) reports palynological studies conducted in the Niger Delta which showed an increase in the occurrence of pollen from the oil palm (indicative of clearing of the primary forest), and a corresponding increase in the occurrence of pollen from certain weeds and other plants consistent with tilling, around 2800–3000 BP. On this basis, she tentatively suggests that agriculture may therefore have begun in the area during this period. Evidence in this chapter suggesting that agriculture was in a nascent stage for the PDC people, and that increasing agricultural sophistication may have led to the break-up of PDC, may well tie in with this date, lending support to the notion put forth in Williamson (1988) that at least certain aspects of farming were introduced to the Ijoid peoples already in the Delta by new arrivals from the east (or northeast), who became the Central Delta people.

As some of the plant terms whose reconstruction formed the foundation of the Food Complex Hypothesis (Williamson 1970, 1993) have also been included in this chapter, it is possible to evaluate the FCH, i.e. that plant names reconstruct differently according to the food plant complex – indigenous, Southeast Asian, and New World – each is associated with. This

hypothesis is a reasonable one in principle, though the evidence offered here lends it only partial support. Terms for indigenous plants which are widely used throughout West Africa and which have presumably been used since antiquity, e.g. bitterleaf and fluted pumpkin, are found not to respect linguistic borders – i.e. having the sort of distribution expected for New World introductions. An attempt to account for this, which concurs with arguments of Williamson (1970) that bitterleaf was introduced to the Delta region through trade, has been offered above. More central to this chapter, while terms for oil palm and raphia palm in Delta Cross seem not to contradict this hypothesis, terms for guinea yam and wateryam, representing the indigenous and Southeast Asian food complexes respectively, are undifferentiated with respect to reconstruction to Proto-Delta Cross.

The FCH would lead us to expect terms for guinea yam, aerial yam and three-leaf yam to reconstruct to PDC, these being indigenous plants. Terms for wateryam and cocoyam would reconstruct to PDC only if this group were young enough, though they should presumably be reconstructible to the proto-languages of the different branches of Delta Cross. These predictions are only partially borne out. Instead, although separate terms for guinea yam and wateryam have been proposed for PDC, the semantic shifting and existence of other terms associated with each makes it unclear as what the precise referents of the reconstructed terms were in PDC. The evidence is especially ambivalent with respect to wateryam. A possible implication of this is that the period of the break-up of PDC may well coincide with the introduction of wateryam to the area or, alternatively, the arrival of PDC speakers in an area where wateryams were already known.[4] In short, while claims concerning the reconstructibility of terms for indigenous plants to a period associated with Proto-Benue-Congo, or earlier, and terms associated with the Southeast Asian complex to a more recent period may appear supported when examining a selection of languages from a variety of linguistic groupings, a closer, bottom-up, examination such as that presented here gives greater evidence of processes of semantic shift, compounding and linguistic borrowing, all of which provide useful keys to culture information, and indicates that the true picture is more complicated. Detailed studies of other related language groups, paying adequate attention to these processes and tying in with sorely needed archaeological and palynological investigations, will further help to clarify the prehistory of this region.

APPENDIX: DATA SHEETS

Wordlists for the various terms discussed in the text, for all the languages for which they were available, are presented in this Appendix. As in the text, an attempt has been made to have transcriptions conform to International Phonetic Alphabet (IPA) conventions, with the exception of /y/, which here is equivalent to IPA /j/. For language data contributed by other researchers, or in some cases by non-linguist native speakers, it has not always been possible to use the standard transcription system, and in many cases material was not tone marked. Inconsistencies in transcription, however, did not present a problem in assessing cognacy of terms.

For reasons of space, certain data for the Upper Cross languages have been omitted here. Only terms for yam, plant (tubers), dig (tubers), hoe (v.), hoe (n.), oil palm, palm oil, and palm wine were available for all (or most) of the Upper Cross group. Where these represent roots different from those found in Mbembe or Doko, they have been included in the discussion in the text. Some terms given in the comparative data sheets are not specifically analysed in the text but are included for the sake of completeness.

Upper Cross

	Yam (gen.)	*Water yam*	*Aerial yam*	*Three-leaf yam*	*Cocoyam (old)*	*cocoyam (new)*
Mbembe	ùxìmá; ìbòm; ètʃê:n	ókí!mɔ́kka			ebubh	
Doko	ètùm / bɜ̀-	ŋetum		detriwi	ɔkara	erheshim
PUC	*-tʷɔ̀k; *-ggʷòŋ; *-dʸén					

Lower Cross

Anaang	ɛ̀diá	àwìrè; àbìdè	úkát	anem; ìfòmò	íkpɔ̀ŋ; ípɔ̀ŋ	asimeka
Ebughu	ɛ̀brɛ̀	àyábrɛ̀		enwinie	ípɔ̀ŋ	animbo
Efai	àbìrɛ̀	ayabre	mpuk	aweni	ŋḿkpɔ̀ŋ	animbo
Efik	bíà	èbìgɛ̀	édómò	énèm; edidia iwa	íkpɔ̀ŋ	
Ekit	ùdíá	àbrè	édómò; ídû	anam	íkpɔ̀ŋ	asimeka
Enwang	ɛ̀brɛ̀	ayabre	mkpuk	aweni	ípɔ̀ŋ	animbo
Etebi	ùdìá	àbrè; ayabre	idu	anam	íkpɔ̀ŋ	
Ibibio	ùdíá	àbìrɛ̀	ídôm; ídómò	ánêm	íkpɔ̂ŋ	àsìmékà; ànìmbò
Ibino	àbíân	àbrè			íkpɔ̀ŋ	
Ibuoro	bíà				ŋḿkpɔ̀ŋ	
Iko	àbíàn				íkpɔ̀ŋ	
Ilue	ɔ̀bìrɛ̀	ɔ̀bìrɛ̀; ayabere		elilue iwe	íkpɔ̂ŋ	asimeke
Ito	bíà	èbìgɛ̀	edime	enem		
ItuMbuso	bíà	èbìʀɛ̂			ŋḿkpɔ̂ŋ	
Nkari	ábíâ	èbìgɛ̀	edom	enem		
Obolo	úkʷá	úkʷá mûmúŋ	írôm	énèm; ìgʷá	íkpɔ̀ŋ; gbáŋ	ɔ̀kpɔ́tɔ̀mìòɔ̀ŋ
Okobo	àbrɛ̀	àbrɛ̀; aya	édò	anom	ípɔ̂ŋ	enimbo

	Yam (gen.)	Water yam	Aerial yam	Three-leaf yam	Cocoyam (old)	cocoyam (new)
Oro	ɛ̀bə̀rɛ̀	àyábə̀rɛ̀	ŋmkpuk; edo	oweni	íkpɔ̂ŋ	onimbo
Uda	ɛ̀bə̀rɛ̀	ɛ̀bə̀rɛ̀			ípɔ̂ŋ	
Ukwa	àbíà	èbìgɛ̀	édómò	iwa ilak eto	ŋníkpɔ̀ŋ	
Usakade	ɔ̀-βɛ̀rɛ̀ / i-	ɔ̀-βɛ̀rɛ̀ / i-			à-sɔ́ŋɔ́ / e-	
PLC	*-bɛ̀dɛ̀; *-bíàn; *-díá	*-bɛ̀dɛ̀; *-ɟá	*-dómò	*-nèm; *gʷeni	*-kpɔ̀ŋ	

Ogoni

	Yam (gen.)	Water yam	Aerial yam	Three-leaf yam	Cocoyam (old)	Cocoyam (new)
Eleme	ɛ̀saà	èburalé; ebura	àdṹ	òtʃṹ	ètòo	
Baan (Goi)	saà	ŋ̀kaŋ̀ká	nsaà	túú	ńdɛ	
Bodo (Gokana)	gjã́ã́	gjã́ã́ya	tʃyá	túú	tógò súgú	
Nweol (Gokana)	gíá	saa		túú	tógò súgú	
Tai	hyaà	àkɔ́rɔ́tɔ́	sia	túú	ńdɛ̀	
Yeghe	zĩ́á	yáà	sìa	túú	ńdɛ̀	
Ken-Kana	zĩ́á	yā	tia	túú	gɛɛ̀rɛ; ídɛ̀	
Norkana	zĩ́á	yaà	tia	túú	ɛdɛ̀ ndɛ̀	
POg	*ɟĩ́ã́			*túú	*-dɛ̀; *-togo	

Central Delta

Abuan	ɛ̀l̩ɛ̀l; ìl̩ɛ̀l	àbìlà	ànóm	òɗówò	òkòlò
Bukuma	ɛ̀lɛ̀[l]				
Obulom	(ɛ̀)ɛ̀lɛ̀ɨ̀n				

	Yam (gen.)	*Water yam*	*Aerial yam*	*Three-leaf yam*	*Cocoyam (old)*	*Cocoyam (new)*
Odual	ɛ̀lɛ̣̀l				ʊ̀ɓɔ̀kɔ̀; àrà	
W. Ogbia	ɛ̀lɛ̀l; zà				òkɪ̀ɗə̀	
Opume	ɛ̀lɛ̀l					
Otuedu	ɛ̀lɛ̀lɨ					
E. Ogbia	ɛ̀l·ɛ̀l				òkòlò	
Kolo	ɛ̀lɛ̀l				òkòlò	
Kugbo	ɛ̀lɛ̀l				òkòlò	
PCD	⋆ɛ̀lɛ̀l				⋆-kòlò	
PDC	⋆-ɟíɛ́n	⋆-bɪ̀dɛ̀	⋆-dóm			

Upper Cross

	Seed yam	*Yam: planting end*	*Yam hair roots*	*Stake for yam*	*Planting mound*	*Make a mound*
Mbembe						
Doko	etum piŋ	etum dero	etum puba	berepiŋ	deyup	piŋ depiŋ
PUC						

Lower Cross

Anaang	uduɔt edia	ibuot edia	ŋkama ebre	ndisa; nnisa	ebono udia	bot ebono ~
Ebughu		uboho ebre	ntitiaŋ ebre	ndise	ebo ebre	yuk ebre
Efai		ubugu ebre	ŋkam abre	ndise	ebo ebre	yuk ebre
Efik	edito abia	ibuoho udia	ŋ́kám á'bíá	ǹdísá	ébómò	bot ebono
Ekit	udo udia	ibuoho udia	ŋwa udia	ndise	ebono	tɔ ebono ~
Enwang	uduk ebre	ubugu ebre		ndise	ebo ebre	yuk ebre

	Seed yam	*Yam: planting end*	*Yam hair roots*	*Stake for yam*	*Planting mound*	*Make a mound*
Etebi	udo udia	ibuoho udia		ndise	ebono	buo
Ibibio	údúɔ̂t ú'díá	íwúòd ú'díá	ŋ̀kám ú'díá	ǹdísá	ébónó ú'díá	fuk udia
Ibino						
Ibuoro						
Iko						
Ilue	emine obere	ubughu obere	ŋkam; idi obere	ndise	ebom obere	ndite obere
Ito	utotobia; uduot abia	ibod bia	ŋkam bia	ndisa	ebine abia	bid ebine
ItuMbuso						
Nkari	uduot abia	ibot abia	ŋkam abia	nduak	emene abia	bot emene bia
Obolo	úkʷá éwôwòp	íbòtúkʷá	ìŋwɔ̀ɔ̀ŋ lék úkʷá	útí úkʷá	óbúbót úkʷá	bót óbúbót
Okobo	uduk	ubughu abre	ŋke	ilu	obe abre	eyoho abre
Oro	uduk ebre	ubughu ebre	ikei ebre	ndise; nsise	ebo ebre	yuk ebre
Uda		ubuo ebere				
Ukwa	uto bia	ibot bia	ŋkam bia	ndisa	ebono bia	fuŋ ebono bia
Usakade						
PLC		*ú-búkòd ~	*ŋkám ~	*ǹdìsá	*ebono	

Ogoni

	Seed yam	*Yam: planting end*	*Yam hair roots*	*Stake for yam*	*Planting mound*	*Make a mound*
Eleme	ɔ̀baa èsaà; òkíkɔ́ ~	ofo èsaà; èbó ésaà	òní ésaà	èté ésũé ésaàrũ	obiboó ésá	borí obibɔ́ɔ́
Baan (Goi)	baà saà	foto saà	ní saà	té saà	kpòbírí saà	túúri saà
Bodo (Gokana)	báà gjã̀ã́	tò	nigjãã	té gjã̀ã́	kpò	nwẽ̀ẽ̀gjãã
Nweol (Gok)	wín gĩa	togógĩa	ní gíá	té gíá	kúmí gĩ́á	nwɛ̀ɛ̀ nu
Tai	kpõ	akobee hyaa	li hjaà	té hyaà	bee bue hyaa	yenè hyaa

	Seed yam	*Yam: planting end*	*Yam hair roots*	*Stake for yam*	*Planting mound*	*Make a mound*
Yeghe	bá zĩ́á	kobee zĩ́á	li zĩ́á	té zĩ́á	bee búé zĩ́á	yerè zĩ́á
Ken-Kana	èbà zĩ́á	ékóbeè zĩ́ã́	li zĩ́á	té zĩ́á	búé zĩ́á	ùb kpò; dɔ̀ bànà
Norkana	aba jía	wo zĩ́a	li zĩ́á	té zĩ́á	bànà	búé kpò
POg	*-báà ~		*ni ~	*té ~		

Central Delta

Abua	ɛ̀l̩ɛ́l íbèp	ɛ̀mʊ́ ɛl̩ɛ̀l	àsɪ́ní ɛ̀l̩ɛ̀l	òréén ɛ̀l̩ɛ̂l	àɗíβûl ɛ̀l̩ɛ̀l	òmúúl à!ɗíβúl
Bukuma						
Obulom						
Odual						
W. Ogbia						
Opume						
Otuedu						
E. Ogbia						
Kolo					èbìnìkà	òkpò èbìnìkà
Kugbo						
PCD						
PDC					*-bon	

Upper Cross

	Plant (tubers)	*Weed (tubers)*	*Dig (uproot tubers)*	*Hoe (v.)*	*Hoe (n.)*	*Handle of hoe*
Mbembe	gè;gbóːn		tùm	tùp; kpâː; yàβà	ìkùp / ǹ-	
Doko	gúːn; tie tuŋ	hun iyiŋ	bò; bu etum	ɸìn; puna; iroi	ɛ̀xɔ̀ːn / bà-; ehɔn	ehɔnere
PUC	*ttʸàní		*tɔ̀ːkí; *llí	*kpádɔ̀; *ttúnì	*-kʊ̀b(b)ɛ́; *-kɔ̀ːttí	

	Plant (tubers)	Weed (tubers)	Dig (uproot tubers)	Hoe (v.)	Hoe (n.)	Handle of hoe
Lower Cross						
Anaang	tɔ́	nam mbiere	ɾɔ̀k; ŋwube	ɾɔ̀k; nám bíɾɛ́	údɔ̀k; óɹɔ̀k	ɔkpɔkɔ udɔk
Ebughu	tɔ́	dip ibie; wak	tíé; buo ebre	tíé; tɔ́	ùgré; ukide	atitie ukide
Efai	tɔ́	wak ibi	dɔ̀k ebre	dɔ̀k; ti isong	údɔ̀ʔ; orɔk	ɔkɔ ɔrɔk
Efik	tɔ́	wàk	dɔ̀k	dɔ̀k; fúŋ	údɔ̀k	ékpɔ́kɔ̀
Ekit	tó		bòkó; dɔ̀	dɔ̀ː; fuŋ; kim	údòʔ	akpoho
Enwang	tɔ́	wak ibi	dɔ̀k	tí ísɔ̀ŋ; ukim isɔŋ	ɔ́ɾɔ̀k	
Etebi	tɔ́		dɔ̀ʔ; bookó	típí; kim	úɾàk	akoh
Ibibio	tɔ́ː	wààk	dɔ̀k; bùkɔ́	dɔ̀k	údɔ̂k	ákpɔ́kɔ̂; úbê
Ibino			dɔ̀k	dɔ̀k	údɔ̀k	
Ibuoro	tɔ́		tíbí	dɔ̀k	údɔ̂k	
Iko	tɔ́		ɾɔ̀k	ɾɔ̀k	údɔ̂k	ítíó úɾɔ̀k
Ilue	tɔ́	wuak ibi	dɔ̀k; ti isoŋ	tí isoŋ	ùkəɾɛ́	eti ukere
Ito	to		buhɔde	fuŋ; kim	údɔ̀ʔ	ɔkpɔ udɔh
ItuMbuso	tɔ́ː		tíbí	dɔ̀k; wàk	údɔ̂k	
Nkari	to	siɔŋɔ	tibi; dɔk	tibi	uduk	ɔkpɔhɔ
Obolo	wǒp	gʷààŋ; gbɔ̂	ɹɔ̀kɔ̂	ɹɔ̀kɔ̂	ɔ́ɾɔ̀k; àsàk; èsìɔ̀	útì ɔ́ɾɔ̀k
Okobo	túá	dib ibi	tíb ísɔ̀ŋ; ebaha abre	ídɔ̀k abre	ɛ́kònò	ɔpɔkɔb udɔk
Oro	tɔ́; bu ebre	kwe ibi; wak	tí; dɔ̀k ebre	dɔ̀k; ti isɔŋ	údɔ̀; ìgɾɛ̀	ɔkpɔhɔ; etu uduk
Uda	tɔ́ː; jòk		tí	tí	ɔ́ɾɔ̀k	
Ukwa	tɔ́		tíbí	kim; kabade	údɔ̀k	ɔkpɔhɔ
Usakade	túɔ́		dɔ̀k	fúŋ	ɛ́-kɔ́ɾì; a-	
PLC	*tɔ́ː	*gʷààk	*dɔ̀k; *tíbí; *bòkó	*dɔ̀k	* -dɔ̀k; *ukide	* ɔkpɔkɔ

Ogoni

	Plant tubers	*Weed tubers*	*Dig (uproot tubers)*	*Hoe (v.)*	*Hoe (n.)*	*Handle of hoe*
Eleme	foó ɛ̀saà	mù èsaà	na ɔnárṹ ésaà	mù; kàbi	ɛ̀tʃwà; ɛ̀kpàbé	èté ɛ̀tʃwà; íʔɔlɔ
Baan (Goi)	fofó saà	kakabi óó	buura sàà	tɔ̀	tɔ̀	tè tɔ̀
Bodo (Gokana)	fò	gbaa vid	bùùrà	gbaa	tɔ̀	tè tɔ̀
Nweol (Gok)	báà nu	kɔɔ vid	bùùrà gĩá		tɔ̀	tè tɔ̀
Tai	whó hyaa	dùa nam	bùgàrà hyàà	dùà	tùà; kpaba	tè tùà
Yeghe	egàrà zĩ́á	zua nam	bugara zĩ́á	tɔ̀	tɔ̀	túm tɔ̀
Ken-Kana	fo zĩa; kē̃b	dùa nám; zùà	gbĩ́; bùgàrà	dùà	tùà	té tùà
Norkana	kérè zíá	jua nám	gbĩ́ jĩ́á	júá nám̃	tùà; íkpàbé; kà tùà	té tùà
POg	*fo		*bùgàrà	*tɔ̀	*-tùà	*èté ~

Central Delta

Abua	ɓéβ; kár	ɣɔ́ɔ̀r; ruβ; tuβ	bùɣá; gúβ	kɔ́r; ɣɔ́; ɣɔ́ɔ̀r	ɛ̀sʊ́à; [à]rʊ́	òòpòβ; òpôβ
Bukuma		tùù	àrímúù	àrímúù		
Obulom	kpéù		bú		ɛ́sʊ̀à	
Odual	ɓeβ	ruβ; tuβ	guβ;gbo	kaê	ɛ̀sʊ̀á; [à]rʊ̀	
W. Ogbia	ànâm	ə̀rûβ	ìgì [m-]	ə̀kpô; ə̀gûŋ	ɛ̀sʊ́à; zà	
Opume						
Otuedu						
E. Ogbia	gbeβ	fɔfɔ	gbo; mugu	n'am	ɛ̀sʊ́á	
Kolo			ògbò; òɓù		ɛ̀sʊ́á	àlòɣòm ɛ̀sʊ́á
Kugbo	gbeβ	papaβ	gbo; ɓu	gu	ɛ̀sʊ́à; àrụ̀	
PCD	*gbeβ		*buga		*ɛ̀-sʊ́a	
PDC			*bùká		*-tùà	

Upper Cross

	Digging stick	*Lot of five yams*	*Lot of ten yams*	*Yam barn*	*Eat first new yam*	*New yam festival*
Mbembe						
Doko	abaere	etum bɔruŋ	etum tup	daban	cha etum sabi	etum sabi ducha
PUC						

Lower Cross

Anaang	ube eto ilɔk	iso udia	iso edia iba	ise edia	nsook edia	nsook edia; usɔrɔ ~ ɔkɔ
Ebughu	ukide			ise	nyafa	
Efai	asim ebre	ebre itiŋ	uti ebre siŋ	utaŋ ebre	nsik ebre	akpo nsik ebre
Efik	eto utimi isoŋ	iso abia	iso abia iba	ísè		úsɔ́rɔ́ abufa abia
Ekit	ateme	isi udia	isi udia iba	ise abia		
Enwang	aisime ebre	ebre itiŋ	uti ebre siŋ	eko	nsik ebre	akpa nsik ebre
Etebi	ateme	isiu udia kiaŋ	isiu udia iba	adiaha udia		
Ibibio	úbê	ísó ú'díá	ísó ú'díá ìbà	ékô ísê; ísê ábìà	ń'súúk; sùì ùdíá	á'kpá ísò ~ ; ń'súúk
Ibino						
Ibuoro						
Iko	ítíó úrɔ̀	àbíàn ìtíòn kí ízít kì		àbíàn lùòp kí ízít kì		
Ilue	eti uti isɔŋ	onyi obere itiŋ	onyi obere luhu		nsek obere	akpa onyi obere
Ito	ateme	isi bia	isi bia iba	isie		usɔrɔ abufa bia
ItuMbuso						
Nkari	ɔduɔ	iso abia	iso abia iba	ise		uwa isɔŋ
Obolo	ɔ́dɔ̀	ábɔ́p úkwá gò	àkɔ̀p ábɔ́p úkwá	ńtóók úkwá	íríèrìè ájá úkwá	ídʒɔ́k ájá úkwá
Okobo	ube abre	ebuk abre itiŋ	ebu abre lugu	ubio abre	akpa iso abre	ndichuai; iyuo
Oro	osim	ute ebere	ebere luoh	eko; ise	nsik ebre	

	Digging stick	Lot of five yams	Lot of ten yams	Yam barn	Eat first new yam	New yam festival
Uda						
Ukwa	isaŋ eto	iso bia ition	iso bia iba	ise		usɔrɔ obufa bia
Usakade						
PLC				*ékò; *ísè	*ńsúúk	*ńsúúk; *úˈsɔ́dɔ́

Ogoni

	Digging stick	Lot of five yams	Lot of ten yams	Yam barn	Eat first new yam	New yam festival
Eleme	ɔ̀dɔ̃	édo èwá èwá ɛ́sa		ògũ̀	òdé ḿbumà èsaà	ògbò ŋ̀já
Baan (Goi)	dɔ			saa kɛkɔri	dé aà saà	
Bodo (Gokana)	gbōi			da	ããgjaa	tôb nu
Nweol (Gok)	dɔ			dá		tôb nu
Tai	dɔ	òʔòò ábá hyaà	lòb ábá hyaa	gúm	de ã́á hyaa	dúá
Yeghe	dɔ	oʔoo ii zĩ́á	lòb íí zĩ́á	da te zia	de aʔaa zia	dee ka zúá
Ken-Kana	dɔ	òʔòò íí zĩá	lòb íí zĩ́á	gúm; dá	dê ã́àʔa zĩ́á	dúá; zúá kānà
Norkana	dɔ			gúm	de ããʔã nu	dúá
POg	*ɔ̀dɔ̃			*ògúm; *dá		

Central Delta

Abua	òbúɣáàm	òòɣ-òòɣ ə̀rìsì ɪ́l̩ɛ̂l	ɗíòp ə̀rìsì ɪ́l̩ɛ̂l	ìkùè ɪ́l̩ɛ̂l	òlé ìmóóm ɛ́l̩ɛ̀l	ɛ̀yáál ípùlə̀ ɪ̀l̩ɛ̀l
Bukuma						
Obulom						
Odual						
W. Ogbia				èdènì; èdénɛ́lɛ̀l		
Opume						

	Digging stick	*Lot of five yams*	*Lot of ten yams*	*Yam barn*	*Eat first new yam*	*New yam festival*
Otuedu						
E. Ogbia						
Kolo	òɓù órérér					
Kugbo						
PCD						
PDC	*-bè					

Upper Cross

	Oil palm	*Young oil palm*	*Mature oil palm*	*Oil palm frond*	*Fibre of frond*	*Leaves of frond*
Mbembe	ù-jì; ɔ́tárá					
Doko	ùtɔ̀; bù-	utɔ piŋ	ire	utɔ iboi	utɔ iboi	itoŋ eyuŋ
PUC	*-ddí; *-tat[t]á					

Lower Cross

Anaang	íták áyòp	uten; nto ayop	akon ayop	ubak ayop	efaaŋ ayop	ndak ayop; ŋkɔŋ ~
Ebughu	ájè	utɔ	ájè	apin	apin	mbak aje
Efai	éjè					
Efik	éyòp	útén	ókòn	ndak eyop		ńdàk
Ekit	áyì	uten ayin	akon ayi	ndaʔ ayi	akpon ayi	ndaʔ ayi
Enwang	ájì	utɔɔ	ɔrɔ aji	ubak aji	efaaŋ aji	nlak aji
Etebi	ájì	ntro ajin	akwɔŋ aji	ndaʔ aji	mfaŋ aji	
Ibibio	áyôp	útén áyòp	ákóón áyòp	úbâk áyòp	éfááŋ áyòp	ńdâk áyòp
Ibino	otʃop	ochio ochop	akwa itio otʃop	upak otʃop		mfaŋ otʃop

	Oil palm	Young oil palm	Mature oil palm	Oil palm frond	Fibre of frond	Leaves of frond
Ibuoro	ójòp					
Iko	àtá ɛràn					
Ilue	ńdígì	utɔɔ	ńdígì		efaŋ ndighi	nlak ndighi
Ito	eyip	uten eyip	eyip	ndak eyip	mfaŋ	
ItuMbuso	ójòp					
Nkari		uten	okon ~ ; odɔt ~	ndak eyop	ɔkɔk eyop	ndak eyop
Obolo	kóòk; kóò	útén; ɡʷúŋ kóòk	kóòk	ńlàk kóòk	ŋkáàràŋ	íŋwâŋ ńlàk
Okobo	áyì	itɔ	ayi	apen ayi	efaŋ ayi	mfaŋ ayi
Oro	ósì	utɔ osi	osi	mbak osi	efuŋ	mbak ayaŋ; nlak osi
Uda	ádʒì					
Ukwa	éjòp	uten eyop; nkehe ~		eyop	ndak eyop	ufaŋ ndak
Usakade	útɛ́n					
PLC	*éɟòb	*ú-tɛ́n	*ókóón	*úbàk ~; *ńlàk ~	*-fááŋ	*ńlàk ~

Ogoni

	Oil palm	Young oil palm	Mature oil palm	Oil palm frond	Fibre of frond	Leaves of frond
Eleme	ajo	ɔ̀ãrã́a àjo	ɔ̀tɔ̀ɔ̀ mɡbɔ́ɔ́	ɔ̀la ajo	ɛ̀fá ɔ́la ajo	
Baan (Goi)	zoo	wĩ́ì zoo	te zoo	la zoo		pa sã zoo
Bodo (Gokana)	ɡjoo	ãã ɡjoo	te ɡioo	là ɡjoo	kul ɡjoo	lâ ɡjoo
Nweol (Gok)	ɡioo	win ɡioo	te ɡioo	lâ ɡioo	kɔ ɡioo	
Tai	zoo	aŋana zoo	ka te zoo	lâ zoo	azẽẽ zoo	apahya zoo
Yeghe	zoo	aŋana zoo	ka te zoo	la zoo	fee zoo	la zoo
Ken-Kana	zoō	aŋana zoo	té zoo	lā zoo	ézẽe zoo	èpà lâ zoo

	Oil palm	*Young oil palm*	*Mature oil palm*	*Oil palm frond*	*Fibre of frond*	*Leaves of frond*
Norkana	zóo	aŋana zoo	té zoo	la zoo	ènɔɔ̀ zoo	yã zoo
POg		⋆ɟoo	⋆aŋana ~	⋆-té ~	⋆-la ~	
Central Delta						
Abua	òrééɲ ə̀lè; [ə̀]rè-	ɔ̀rìr; ɔ̀rìì	àlhè; ə̀lè	ɔ̀kálágùa; àɗíɣɔ́ɔ̀ ~	ạaphù; ɛ̀kpólóɣì ~	ríɲá ə̀lè
Bukuma	òlè					
Obulom	ògùlè					
Odual	ə̀lè			òòɗùm; ìì-		
W. Ogbia	ə̀ɗê; zə̀					
Opume						
Otuedu						
E. Ogbia	ə̀ɗè					
Kolo	àɗè	ɔ̀rìr	àtèn mááɗè	ògólògò áɗè		
Kugbo	ə̀ɗè					
PCD	⋆-ɗè					
PDC	⋆-dde; ⋆-ɟòb; ⋆-tén				⋆-lák	⋆-lák

Upper Cross

	Palm fruit cluster	*Stem of cluster*	*Single fruit*	*Red palm oil*	*Oil pressing trough*	*Palm kernel*
Mbembe				ànà		
Doko	dirheruwen	deraŋ	derheyup	ɛ̀dɔ̀ːmì	ekpuha	eyep
PUC				⋆-ntànà; ⋆-nɔ̀ːmì		

	Palm fruit cluster	*Stem of cluster*	*Single fruit*	*Red palm oil*	*Oil pressing trough*	*Palm kernel*
Lower Cross						
Anaang	ifen(e) ayop	ŋkɔm ayop	ŋkua ayop	árân	akwa	isip
Ebughu	utu aje	oputo	aje	ádâ	akwa	ísíp êjè
Efai				ádà		í-síp éyè
Efik	ìfèn ~	ífiet	éyòp	ádàn	ákwá	ísíp
Ekit	ifene ayi	akpon	ayi	ádè	akwa	ísíp éyì
Enwang	utu aji	upuk aji; oputie	usio aji	ádà	akwa	ísíp úbíò
Etebi				ádèʔ		ísíp ú'táŋ
Ibibio	ìfèn áyòp	ákpê áyòp; ákúêt ~	ŋ̀kùà áyòp	ádân	ákúá	ísíp
Ibino				ɛ́dàn		íkíp
Ibuoro				ádàn		ísíp
Iko				ɛ́dân		íkíp
Ilue	otu ndighi		mkpasip ndighi	ádâ	okpono	ísíp ~
Ito	ifen ~	ifid eyip	mkpata eyip	árên	akwa; uduŋ isɔŋ	isip eyip
ItuMbuso				ádân		íkíp
Nkari	ifene eyop	ifiet	mkpata eyop		akwa	isip
Obolo	étò; ìfèn kóòk	ódùdû	íbòt kóòk	áràŋ	ákwá; ókwá árâŋ	ítʃíp íkpɔ́k
Okobo	utu ayi	akuen ayi	iyip anyi	ádâ	akwa	ítʃíp ɔ́jì
Oro	otu osi	ukpu osi	ukup osi; ukip ~	ádà	akwa	íkíp ósì
Uda				ádâ		ísíp
Ukwa	ifene eyop	ifiet	mkpata eyop	ádân	akua	ísíp
Usakade				ó-dân / i-		ó-síp / í-
PLC	*-fèn; *-tò			*ádàn	*akúá	*íkíp

Ogoni

	Palm fruit cluster	*Stem of cluster*	*Single fruit*	*Red palm oil*	*Oil pressing trough*	*Palm kernel*
Eleme	èkũ ajo	èkpíkpíí ájo	òʔí ájò	nnɔ́ɔ	ɛ̀kwã́	èʔí
Baan (Goi)	kɔ̀ zoo	kpúkpúí zoo	boó zoo	nɔ́ɔ zoo	kpòjɛ̀ɛ̀	i i
Bodo (Gokana)	kùm gjoo	kpig gjoo	kpoò gjoo	nɔ́ɔ		ib
Nweol (Gok)	kum gioo	kɔ̀ gioo	kpóó gjoo	nɔ́ɔ gioo	bii gioo	ib
Tai	kum zoo		bee zoo	nɔ́ɔ zoo		ib
Yeghe	kum zóo	gbî zóo	beè zóo	nɔ́ɔ zóo	kúi zoo	ib
Ken-Kana	kum zóo	gbî zóo	beè zóo	nɔ́ɔ zóo	kuã zóo	ib
Norkana	èkɔ̀ zóo	gbî zóo	bee zoo	nɔ́ɔ	kà kúã́	ib
POg	*-kum	*-kpik	*-bee; *-kpo	*-nɔ́ɔ ~	*-kua	*-ʔib

Central Delta

Abua	èɣúùn ə̀lè	òtúm ə̀lê	èkpó ə̀lê	àmùnùm; àmʊ́nʊ́m	òɣúúɣ ə̀lè	èkpézíβ; èkpò ézîβ
Bukuma						
Obulom				àmʊ́nʊ́m		
Odual						ékpózìm
W. Ogbia						òzù
Opume						
Otuedu						
E. Ogbia				àmʊ̀nʊ̀		òzù; ì-
Kolo	ègún áɗé	ègùm áɗè	ònín ègún	àmʊ̀nʊ̀	ɔ̀kpɔ̀ìn	ìzùβ
Kugbo						òzìβ
PCD				*àmʊ́nʊ́m		*-zib
PDC			*-ɟòb	*-nɔɔm; *-tàn	*-kua	*-kib

Upper Cross

	Oil from kernel	*Broom from frond*	*Broom from stem*	*Tap wine*	*Palm wine tapper*	*Oil palm wine*
Mbembe						
Doko	idemia	ituehe	ituehere	pɔ utɔ	opi-utɔ	ìrùyà; bòròyà; eruabom
PUC						

Lower Cross

Anaang	m̀mànyàŋà	ayan	ape ayop	tuak ukɔd	apik ukot	ú-kɔ̀ɾ ŋ́ʷɔ̀ŋ
Ebughu	m̀mànyàŋà	ayanyaŋa	oputo	tuak	atuak uko	ḿ-mí ɛ̂dʒɛ̀
Efai						ḿ-mí ɛ̀fɛ̂k
Efik	m̀mànyàŋà	áyàŋ	akpo ayaŋ	kpí ~ ; tuak ukɔt	èkpì; atuak ukɔt	ḿmín éyòp
Ekit	mmayaŋa	ájâŋ	ajaŋ	tou uko	atuaʔ uko	úkò ájì
Enwang	m̀mànyàŋà	inyaŋa	mkpai aji		atuak uko	ḿ-mí ɛ́fɛ̀k
Etebi						ú-kò ékìt
Ibibio	m̀mànyàŋà	áyâŋ; ífiot	ákpê áyòp; ákpît	tùàk ~ ; kpí ~	àkpì	úkɔ̀t
Ibino	mmanyaŋa	ndiak	ayaŋ	tuak	atuak mmiŋ	mmiŋ otʃop
Ibuoro						ú-kɔ̀t ḿmùŋ
Iko		ázàŋ; ífió				à-tá m̀mìm
Ilue	m̀mànyàŋà	ayaŋ	nlak ndighi		atuak uko	à-táí m̀mì
Ito	mma - yaŋ	ayaŋ		tuak ukɔd	atuak ukɔd	ukɔd eyip
ItuMbuso						ú-kɔ̂t; ~ ójòp
Nkari	mmanyaŋa	ayaŋ	eyeya	bi ukɔt	ebei ukɔt	ukɔt eyop
Obolo		íŋwâŋ ájíjà; àzízàŋ	ùkwút ádʒídʒà	tʃà!k míìŋ	ògwú òtʃák mììŋ	míìŋ kóòk
Okobo	m̀mànyàŋà	áyâŋ	ufibi		ati uko	ḿ-mí ɛ́fɛ̀k
Oro	m̀mànyàŋà	nda osi; anyanyaŋ	ayaŋ	tuak mmi	okpi; atua mmi	mmi osi

	Oil from kernel	Broom from frond	Broom from stem	Tap wine	Palm wine tapper	Oil palm wine
Uda						ɛ̀-kɛ́ ḿmì
Ukwa	mma - yaŋ	ndak	ayaŋ	bi ukot	ebi ukɔt	ú-kɔ̀t
Usakade						è-míŋ ájòp
PLC	*m̀-mànyàŋà	*á-ɟàŋ				*émín ~

Ogoni

	Oil from kernel	Broom from frond	Broom from stem	Tap wine	Palm wine tapper	Oil palm wine
Eleme			ǹsísárí	âʔ mmí	owĩ́ ɔ̀ʔã mmí	m̀mí ájo
Baan (Goi)			sàsàì	aa mĩ́ĩ́	nee aa zoo	míí zoo
Bodo (Gokana)				ali mĩ́ĩ́		mii gjoo
Nweol (Gok)		bɔ́b sã̀	kpóó sã̀	ari míí	nee ari kɔl	mii gioo
Tai		bɔ́b asã̀	akpo asã	ee míí	nee ee kue	míí zóo
Yeghe		bɔ́b sã̀	kpo sã	ae kuɛ	nee ae kuɛ	míí zóo
Ken-Kana			ekpó èsã̀; àsàà̀	áɛ́ míí	nɛɛ̀ áɛ̀ kuɛ	míí; kūɛ̄
Norkana				áɛ́ kuɛ	nɛɛ̀ áɛ̀ kuɛ	míí zóo
POg				*ari ~	*ne ~	*mmii ɟoo

Central Delta

Abua		ɔ̀zànì	ɔ̀zànì	gbó; ògbù óɣôl	ogbom ighol	ə̀míìm; àmíím ə̀lê
Bukuma						
Obulom						èmìmʊ̀ ésébè
Odual						
W. Ogbia						ə̀mì
Opume						

	Oil from kernel	*Broom from frond*	*Broom from stem*	*Tap wine*	*Palm wine tapper*	*Oil palm wine*
Otuedu						
E. Ogbia						əmìn
Kolo			ɔ̀zàŋànì	òsòβ ídè	òsòβ mà ɔ̀ɣɔ̀	àmìn
Kugbo				gbo		əmìn ~ əmìm ùdè
PCD				*gbo ~		*e-mim ~
PDC		* -ɟàŋ				*-mim ~

Upper Cross

	Raphia palm wine	*Raphia palm*	*Young raphia palm*	*Mature raphia palm*	*Raphia frond*	*Raphia fibre*
Mbembe						
Doko						
PUC						

Lower Cross

Anaang	ukɔt	ú-kɔ̀t	isip ukɔt	ikporo ukɔt	ɔkɔk; ndak ukɔd	ndaam
Ebughu	ukɔ	ú-kɔ̀	ukɔ	ukɔ		
Efai		ú-kɔ̀				
Efik	ńímín	ú-kɔ̀t				
Ekit	úkó ɛ́kɛ̀t	ú-kò	mkpok uko	akpoho uko	ndaʔ uko	ndam
Enwang		ú-kɔ̀	uyo ukɔ	apei uko	apin	nla
Etebi		ú-kò				
Ibibio	úkɔ̂t	ú-kɔ̂t	útén úkɔ̀t	ákóón úkɔ̀t	ákɔ́ɔ̀k; ńdâk	ńdáám
Ibino	ukɔd mmiŋ	ń-láːm	nsek ukɔd	ikpɔ ukɔt	ndiak ukɔd	nlam
Ibuoro		ú-kɔ̂t				

	Raphia palm wine	*Raphia palm*	*Young raphia palm*	*Mature raphia palm*	*Raphia frond*	*Raphia fibre*
Iko		ǹ-ɲàŋ m̀mìm				
Ilue	mmi ukɔ	ú-kɔ̀	ŋkeŋe ukɔ	ukɔ	opin; okuko	nla
Ito	ukɔd	ukɔd		ukɔd	ndaʔ ukɔd	ndaam
ItuMbuso		ú-kɔ̂t				
Nkari	ukɔt	itak ukɔt	iyen ukɔt	itak ukɔt	ndak ukɔt	ndam
Obolo	míiŋ òbòlò	úgɔ̀t; míiŋ	gwúŋ úgɔ̀t	úgɔ̀t òbêbèt	ńlàk úgɔ̀t	ńráàŋ
Okobo	mmi ukɔ	ukɔ	uyo ukɔ		mbak ukɔ	nla (idrit)
Oro	mmi ukɔ	ú-kɔ̀	uyu ukɔ; ŋkeŋe ukɔ	okpono ukɔ	okpin; nlak ukɔ	nla ukɔ
Uda		ú-kò				
Ukwa	ukɔt	ú-kɔ̀t	nsek ukɔt	akaba ukɔt	ndak ukɔt	ndam
Usakade		á-kò / ú-				
PLC	*émín úkɔ̀d; *úkɔ̀d	*-kɔ̀d			*ńlàk úkɔ̀d	*ńláám

Ogoni

	Raphia palm wine	*Raphia palm*	*Young raphia palm*	*Mature raphia palm*	*Raphia frond*	*Raphia fibre*
Eleme	m̀mí ŋ́kõ	ŋ́kɔ̃	egbo ŋkɔ̃	ŋ̀kɔ̃ rẽe abíni	ɔla ŋkɔ̃	ɛ̀sɔɔ̀; ɛ̀suɛ̀
Baan (Goi)	míí ŋ́kɔɔ	ŋ̀kɔɔ	wĩ́i ŋkɔɔ	té ŋ́kɔɔ	la ŋkɔɔ	mímo ŋkɔɔ
Bodo (Gokana)	míí kɔl	kɔl	aa ŋkɔɔ	tɛ́ kɔl	la kɔl	kpẽ
Nweol (Gok)	mii kɔl	kɔl	wín kɔl	té kɔl	la kɔl	sibè
Tai	mii kuɛ	kuɛ	wĩ́i kuɛ	ka té kuɛ	la kuɛ	akwere
Yeghe	mii kuɛ	kuɛ	mɛɛ íí kuɛ	té kuɛ	pa la kuɛ	mue kuɛ
Ken-Kana	mii kuɛ; míí kānà	kuɛ	ekéé kuɛ	té kuɛ	la kuɛ	kúrè

	Raphia palm wine	*Raphia palm*	*Young raphia palm*	*Mature raphia palm*	*Raphia frond*	*Raphia fibre*
Norkana	míí kuɛ	kuɛ	wĩ́ĩ kuɛ	té kuɛ	la kuɛ	kúrè
POg	⋆mmii ŋkɔl	⋆ŋkɔl			⋆-la ŋkɔl	

Central Delta

Abua	míím íyâ	ɔ̀ɣɔ́l	òɣól	òɣól	àɗíwâ	í!pé
Bukuma						
Obulom						
Odual		ɔ̀ɣɔ́l; ɔ̀gbɔ́				
W. Ogbia		ɔ̀ɣɔ́l; èɗúmɔ̀ɣɔ́l				
Opume						
Otuedu						
E. Ogbia		ɔ̀ɣɔ̀l; òɗùɗúm				
Kolo	ìdè					ɔ̀ɣɔ́l
Kugbo		ɔ̀ɣɔ́l; òɗùɗùm				
PCD		⋆ɔ̀ɣɔ́l				
PDC	⋆-nim ~	⋆ -kɔd			⋆-làk	

Upper Cross

	Raphia fruit	*Kernel of fruit*	*Raphia swamp*	*Bitterleaf*	*Fluted pumpkin*	*Pumpkin leaf*
Mbembe						
Doko				otidot	ekumidipum	ekumidiyuŋ
PUC						

	Raphia fruit	Kernel of fruit	Raphia swamp	Bitterleaf	Fluted pumpkin	Pumpkin leaf
Lower Cross						
Anaang	ŋmkpa ukɔt	ube; isip ukɔd	epen ukɔt; mbad ukɔd	àtídòt	ùbɔ́ɔ́ŋ; ifere	ŋkɔŋ ubɔŋ
Ebughu	aya		adie	etiluo		ifaŋ ubre
Efai			adi	etiliu	awak	ŋkie
Efik	ube			ètídòt	ùbɔ́ŋ	ikɔŋ ubɔŋ
Ekit	mkpa uko; aya	mkpasi aya	ndua	ètídòt	ùbɔ́ŋ	ŋkɔŋ ubɔŋ
Enwang	aya	usip	adi	atilio	awak	ŋkie
Etebi						
Ibibio	m̀kpó úkɔ̀t	ùbê	ńdúá; àdìk úkɔ̀t	àtídòt	ùbɔ́ɔ́ŋ	ŋ́kɔ̂ŋ
Ibino		ikib ukɔd	mpad ukɔd	atilod	ubɔŋ	mfaŋ ubɔŋ
Ibuoro						
Iko				àlúbɔ̀rɛ́		
Ilue	isip ukɔ	aya ukɔ	adi ukɔ	otilu	ubere	mfaŋ ubere
Ito		isip	ndua	etidit	ubɔŋ	ikɔŋ ubɔŋ
ItuMbuso						
Nkari	mkpɔ ukɔt	mkpɔ ukɔt	edep	etidot	ubɔŋ	nnanyi
Obolo	ḿfùt úgɔ̀t	ítʃíp úgɔ̀t	ńríá úgɔ̀t	ǹtìló; òlúbìrì; àfìlàmà	m̀bɔ́ɔ́ŋ; ùbɔ́ɔ́ŋ	íŋwâŋ m̀bɔ́ɔ́ŋ
Okobo	mkpok ube	nditʃip ube	ade	àtílúó	nditʃip m'buɔ́ŋ	mfaŋ mbuɔŋ
Oro	aya	ukpe aya; ukip aya	ɔdi; mbe	atilo; otilu	awak	ifaŋ ubre
Uda					ofiop	
Ukwa	ekin ukɔt	mkpɔ ukɔt	edep	etidot	ùbɔ́ŋ	
Usakade						
PLC				* -tílòd	*ù-bɔ́ɔ́ŋ	

Ogoni

	Raphia fruit	*Kernel of fruit*	*Raphia swamp*	*Bitterleaf*	*Fluted pumpkin*	*Pumpkin leaf*
Eleme	òʔí ŋ́kɔ̃		ɔ̀kɛ̃	òlígbò	ógũ	ǹsógũ
Baan (Goi)	bóó ŋkɔɔ		ɔɔ ŋkɔɔ	m̀gbɔ̀rɔ̀kí	m̀ʔéé	kpɔ́
Bodo (Gokana)	bol kɔl		ɔɔ	orobiri	éb	sã éb
Nweol (Gok)			ɔɔ	orobiri	éb	sã éb
Tai	beè ala kuɛ		ɔɔ	olubiri	éé	hya éé
Yeghe	bee ii kuɛ	ib	ɔɔ			
Ken-Kana	ib kuɛ		ɔ̀ɔ	èlobiri	éé	yã éé
Norkana	ib kuɛ		ɔ̀ɔ		éé	yã éé
POg				⋆-lobiri	⋆-eeb	

Central Delta

Abua	òbógùsì	èkpò òbógùsì	ìkpágì iɣóòl	ʊ́kɪ̀ɔ̀	ééssé	ɔ́kʊ́sswɔ́
Bukuma						
Obulom						
Odual				ʊ́kɪ̀ɔ̀		
W. Ogbia				əɗírí ɔ́sɛ́		
Opume						
Otuedu						
E. Ogbia				òlúgbò; əɗírí ɔ̀sɔ́		
Kolo	òɓósù ɔ́ɣɔ́l	ɔ̀kpɔ̀lɔ̀kpɔ̀ òɓósú	àbàrà	àdìrì ɔ́sɔ̀	ɔ̀gʊ́	ətò ɔ̀gʊ́
Kugbo						
PCD						
PDC	⋆-kib; ⋆-be	⋆-kib; ⋆-be				

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NOTES

1 Williamson (1993: 140) says names associated with plants of Southeast Asian origin could 'not be reconstructed to the proto-language of a group . . . but rather gave the impression of loan-words which had come into use after the proto-language had begun to split up into the modern languages'. On this basis it is difficult to see how one differentiates clearly on a linguistic basis between plants of Southeast Asian origin and those of New World (or European) introduction; the broad rather than detailed scope of data presented in that paper does not in any case permit reconstruction to a node lower than Proto-Benue-Congo. It seems fair to assume that, if there is any distinction between plants of Southeast Asian origin and those of New World introduction, it should be in the ability to reconstruct the former at least to a low node on the tree – in the present case possibly to Proto-Delta Cross or at least the proto-languages of its four constituent branches.

2 Williamson (1993) suggests the *-kpɔ̀ŋ root is cognate with forms that are widespread in Benue-Congo, but in the absence of other evidence to support the required phonological developments, its cognacy with these forms can be seen only as speculative at best. The fact that this term does not reconstruct to PDC presents an additional problem for its inclusion in Williamson's list.

3 There are other examples of a possible correspondence between PLC *f and POg *k, though none is particularly convincing of a sound shift.

4 Blench (n.d.) suggests a direct introduction of wateryam and other aspects of, broadly speaking, Southeast Asian/Pacific culture to West Africa. Exploration of this issue is beyond the scope of this chapter. However, an obvious route for research would be to look for possible connections among lexical items used for food and cultural artefacts that are candidates for such an introduction and the terms for corresponding items among Malayo-Polynesian languages at the approprite level of reconstruction. Finding such correlations would provide persuasive evidence for Blench's hypothesis, as well as giving further important insight into the development of yam culture in Southeastern Nigeria.

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15 *Japanese rice agriculture terminology and linguistic affiliation of Yayoi culture*

ALEXANDER VOVIN

ABBREVIATIONS

CK	Ceycwu dialect of Korean	OJ	Old Japanese
HK	Hamkyeng dialects of Korean	OT	Old Turkic
J	Modern Standard Tokyo Japanese	PA	Proto-Altaic
K	Modern Standard Seoul Korean	PAN	Proto-Austronesian
KA	Kagoshima dialect of Japanese	PJ	Proto-Japonic
KY	Kyoto dialect of Japanese	PK	Proto-Korean
MC	Middle Chinese	PM	Proto-Mongolic
MJ	Middle Japanese	PMT	Proto-Manchu-Tungus
MK	Middle Korean	PT	Proto-Turkic
NJ	Nakijin dialect of Ryukyuan	SH	Shuri dialect of Ryukyuan
OC	Old Chinese	WM	Written Mongolian

INTRODUCTION

Both archaeologists and linguists still continue to argue regarding the time of migration of the Japanese people to the Japanese islands. Among the three major prehistoric archaeological cultures in Japan in the first millennium BC and the first millennium AD – Jômon[1] (until 3rd century BC), Yayoi (third century BC to third century AD), and Kofun (third century AD to fifth century AD) – the Japanese are most often associated with the Yayoi culture. The majority of archaeologists, historians and linguists support this point of view (Egami 1967; Ledyard 1975). However, some scholars, especially linguists, have expressed other opinions. R.A. Miller (1980, 1986a, 1986b) is probably the only scholar who believes that the Japanese language may go as far back as the Jômon culture, while the more common opinion is that Jômon culture should be associated with Ainu, the aboriginals of the Japanese archipelago. Another point of view suggests that the Japanese were the representatives of the Kofun culture (Unger 1990). The goal of this chapter is to support this

last point of view, to demonstrate that linguistically the Yayoi population cannot be defined as Japanese-speaking. This implies a question about the linguistic identity of the Yayoi people: if Jômon is associated with the Ainu and Kofun with the Japanese, who then were Yayoi from a linguistic standpoint? I shall try to provide an answer to this question.

At first glance there is strong evidence that Japanese was already spoken in Japan by at least the end of the Yayoi period, since several titles and personal names recorded by Chinese envoys to Wa country (AD 269) in 倭人伝 (Wo ren zhuan) [ʔwa njin trjons] section of the 魏志 (Wei zhi) [nguj tjij] 'Wei History' are believed to be Japanese. Chinese pronunciation in the third century AD was still very close to Old Chinese (OC), and reading these characters according to their Man'yôgana phonetic values, reflecting to some degree the Middle Chinese of seventh–eighth centuries AD, is an anachronism. Besides, these are words recorded by Chinese, and attaching Man'yôgana readings to them is absolutely inappropriate. The majority of these glosses are hardly identifiable at all, but there are several words which are usually taken for granted as examples of 'Japanese' words;

> The title 卑奴母離 is believed to reflect Japanese **pyi no mori* 'guard of the sun(?)', even including the genitive marker *no*, suggesting that it is a sample of the Japanese language. However, in the third century AD the Chinese reading of the characters above was **pji-naʔ-mə-rjaj*, which makes equation with tentative Japanese **pyi no mori* at least doubtful.

> The title 卑狗 is usually interpreted as Old Japanese (OJ) *pyikwo* 'prince', lit. 'son of the sun'. However, this interpretation becomes unrealistic, too, as soon as we take into consideration the Late Old Chinese reading of these two characters: **pji-kros/ʔ*, where cluster **kr-* would hardly have been used to transcribe plain Japanese *-k-* in *pyikwo*.

> The name (title?) of the supreme female ruler, 卑弥呼 [pji-mjejʔ-hwa], usually interpreted as Japanese *pyi-myikwo* (Himiko) 'sun priestess', may be the best bet for those who would like to see the language of Yayoi as 'Japanese', but still a reasonable explanation for the Chinese transcription of a simple Japanese syllable [mi] with [mjejʔ] would be in order.

If the Yayoi people were not the speakers of Proto-Japanese, who then were they linguistically? The glosses above recorded by Chinese in the third century are not reliable, because in the majority of cases we do not actually know what they mean: therefore their interpretation opens the way for wild speculation. However, the conjunction of archaeological and linguistic data may produce a more convincing response. There is a wide consensus among archaeologists that the people who brought Yayoi culture to the Japanese islands in the third century BC also brought with them rice agriculture. This

culture is believed to have come from coastal Southeast China to Kyûshû (Egami 1967: 329ff.). Therefore, it is reasonable to expect that rice terminology in Japanese or at least part of it will be of Yayoi origin. The historical linguist's task is then to determine the etymologies of rice agriculture terms in Japanese, tracing the origin of these terms the language family in the region where they originated. This should provide useful indicators as to the linguistic affiliations of the Yayoi culture.

Since Yayoi originated somewhere in coastal Southeast China, it leaves us with two more probable choices:

- Yayoi were an Austronesian-speaking people
- Yayoi were an Austroasiatic-speaking people

and two less probable:

- they were Tai-speaking people
- they were Miao-Yao-speaking people

This chapter explores these possibilities by seeking some etymologies in Japanese rice agriculture terminology in these language families or alternatively demonstrating that there are no such etymologies.

The alleged Japanese–Austronesian connection enjoys some degree of recognition on a popular level, with some scholars even claiming the existence of an Austro-Japanese 'family' (Benedict 1990). I have demonstrated that the Austronesian theory of Japanese origins is untenable (Vovin 1994a); moreover, I doubt that there ever was even an Austronesian substratum for Japanese (Vovin 1994b). Even the linguistic evidence that the Payato and Kumaso tribes in Kyûshû were Austronesian-speaking is meagre in the case of Payato and non-existent for Kumaso. Among three major successive prehistoric archaeological cultures in Japan – Jômon, Yayoi, and Kofun – hypothetical Austronesians can fit into the Yayoi only. If the Yayoi people *were* Austronesians who brought rice agriculture to Japan, we should expect that rice terminology in Japanese to be of Austronesian origin. The following lexical items in Proto-Japanese connected with rice agriculture are analysed in the tables that follow; none of the basic Proto-Japanese (PJ) words has a convincing Austronesian etymology.

★(z)ina-Ci/★(h)ina-Ci 2.4[2]	riceplant (Table 15.1)
★momi 2.1	unhulled rice (Table 15.3)
★d$\underline{o}$na-Ci 2.1	hulled rice (Table 15.4)
★k$\underline{o}$ma-Ci 2.3	(hulled) rice (Table 15.5)
★ipi 2.3	cooked rice (Table 15.7)
★pwo 1.3a	ear of grain (Table 15.9)
★ta 1.3a	ricefield (Table 15.10)
★nuka ?2.3	rice bran (Table 15.12)
★kwo 1.3a	flour, powder (Table 15.13)
★n$\underline{o}$ri 2.3	starch, rice glue(Table 15.15)

PROBABLE LEXICAL AFFILIATIONS OF JAPANESE RICE AGRICULTURE TERMINOLOGY

Table 15.1 Japonic forms for 'riceplant' (1) PJ *(z)ina-Ci/*(h)ina-Ci 2.4

Family	*Group*	*Language*	*Attestation*	*Gloss*
Japonic[3]	Japanese	OJ	ine/ina-	rice plant
Japonic	Japanese	MJ	ìné[4]	rice plant
Japonic	Japanese	J	íǹe	rice plant
Japonic	Japanese	KY	ìné	rice plant
Japonic	Japanese	KA	ìné	rice plant
Japonic	Ryukyuan	SH	ʔŋnì	rice plant
Japonic	Ryukyuan	NJ	–[5]	–

Commentary 15.1 and 15.2

There are a number of compounds where *ine* appears as *-sine*: *kati-sine* 'unhulled rice', *kuma-sine* 'washed riced offered to the gods', *uru-sine* 'non-glutinuous rice', *mi-sine* 'honourable/beautiful rice', which suggest that in PJ the word started with some consonant which was otherwise lost in initial position, probably *z- or *h-. Benedict (1990: 234), obviously unaware of this, as well as of the fact that this word and *yone* 'hulled rice' < PJ*do̲na-Ci 2.1 (see number (3)) belong to two different accent registers in PJ, and therefore have different origins, lumps them both in one etymological entry. Furthermore, he compares both J *ine* and *yone* with Proto-Miao-Yao *hn[]^{C} = *hnaaŋ 'cooked rice', citing the reconstruction by Purnell (1970: 162). Benedict's *hn[]^{C} is actually a miscitation of Purnell's *(nh___C), and Purnell's *nhaaŋ is a Proto-Yao, not a Proto-Miao-Yao form (Purnell 1970: 162). Table 15.2 represents the attestations of 'cooked rice' in Miao-Yao languages.

Table 15.2 Miao-Yao forms for 'cooked rice'

Family	*Language*	*Attestation*
Miao	Petchabun	nho4
Miao	Proto-West A	*nho5
Miao	Proto-Western	*nho C1(?)
Yao	Chiengrai	nhaaŋ5
Yao	Hsing-an	nhaŋ5
Yao	Tai-pan	nhaaŋ4
Yao	Proto-Iu-Mien	*nhaaŋ5
Yao	Haininh	naaŋ3
Yao	Ling-chun	nhaaŋ7

Moreover, Benedict's further reconstruction *hnaaŋ < *snaŋ is completely *ad hoc*. There is no possibility of reconstructing exactly the quality of the initial consonant in PJ *(z)ina-Ci/*(h)ina-Ci 2.4 'riceplant' on a purely internal basis, but the following external etymology seems to support the variant with

*h-: Proto-Wa *hŋɔʔ 'riceplant' (Diffloth 1980: 49). The semantics of the etymology is also better than Benedict's comparison, since both PJ *hina-Ci and Proto-Wa *hŋɔʔ mean 'riceplant'.

Table 15.3 Japonic forms for 'unhulled rice' (2) PJ *momi 2.1

Group	*Language*	*Attestation*
Japanese	OJ	(momi)[6]
Japanese	MJ	mómí
Japanese	J	mòmí
Japanese	KY	mómí
Japanese	KA	mómì
Ryukyuan	SH	múmì
Ryukyuan	NJ	mùmíí

Commentary 15.3

It is quite possible that this word has an internal etymology, being just an adnominal form of the PJ verb *mom- 'to pound' as suggested by Martin (1987: 484). Therefore, PJ *momi 2.1 can mean 'thing [designated] for pounding', and there is no need to seek an external etymology. Without referring to Martin's proposal, Benedict (1990: 234) compares Japanese *momi* 'unhulled rice' with Atayal *mamiʔ* 'cooked rice'. However, the very fact that this form is isolated in Atayal makes his etymology doubtful. Besides, it is unlikely that there was any influence from prehistoric Taiwanese rice agriculture on Japan via the Ryukyuan islands, since no Yayoi archaeological sites have been discovered on the Ryukyus (Levin 1971: 185).

Table 15.4 Japonic forms for 'hulled rice' (3) PJ *d̲o̲na-Ci 2.1

Group	*Language*	*Attestation*
Japanese	OJ	yo̲ne
Japanese	MJ	yóné
Japanese	J	yòné
Japanese	KY	yóné
Japanese	KA	?yónè[7]
Ryukyuan	SH	?[8]
Ryukyuan	NJ	?[8]

Commentary 15.4

This word belongs to the high accent register, while PJ *(z)ina-Ci/*(h)ina-Ci 2.4 'rice-plant' belongs to the low accent register. Their initial consonants are quite different, excluding any possibility that they are cognates within Japanese. There are no obvious internal or external etymologies for PJ *do̲na-Ci 2.1 'hulled rice'.

Table 15.5 Japonic forms for 'hulled rice' (4) PJ *ko̲ma-Ci 2.3 '(hulled) rice'

Group	*Language*	*Attestation*
Japanese	OJ	kò̲mèy
Japanese	MJ	kòmè
Japanese	J	kòmé
Japanese	KY	kómè
Japanese	KA	kòmé
Ryukyuan	SH	kùmì
Ryukyuan	NJ	hùmìí

Table 15.6 Mon-Khmer words for 'cooked rice'

Group	*Language*	*Attestation*
Viet-Muong	Hanoi Vietnamese	co'm [këm]
Viet-Muong	Saigon Vietnamese	këm
Viet-Muong	Middle Vietnamese	këm
Viet-Muong	Muong	këm

Commentary 15.5 and 15.6

Martin (1987: 455) suggested an internal etymology: 'ko- "plant" + -mey "sprout" or "eye" if not from < Ch[inese] m[b]ei'. However, this internal etymology seems to be semantically forced. His Chinese etymology of -mey runs into a phonetic problem, too: it cannot be a loan of the MC /mej/ '(hulled) rice', since *ko̲mey*, even if it is a compound, was already an obsolete compound in Old Japanese. Therefore, the word should have been borrowed prior to the OJ period, in which case it was certainly borrowed from Old Chinese. But OC reading of the character is *mij, not /mej/, which would yield OJ *-myi*, rather than *-mey*. It is quite possible that PJ *ko̲ma-Ci 2.3 '(hulled) rice' has an old Austroasiatic etymology, proposed by Matsumoto Nobuhiro: Vietnamese *co'm* 'cooked rice' (Matsumoto 1928) < Proto-Viet-Mu'o'ong *këm 'cooked rice' (Thompson 1976: 1183) (Table 15.6).

Table 15.7 Japanese words for 'cooked rice' (5) PJ *ipi 2.3

Language	*Attestation*
OJ	ìpyì
MJ	ìfì
J	íì[9]
KY	íì

Commentary 15.7

No known attestations in KA or Ryukyuan. The word represents an adnominal form *ip-i 'the edible [thing]' of the verb *ip- 'to eat' (cf. Middle Japanese *if-* in Ohno 1940/1974: 128c) < PJ *(d)ip-, comparable with PA *jew- 'to eat' (> PK *cap/b-, PMT *jeb- and PT *ye- 'id.') (Table 15.8).

Table 15.8 Altaic terms for 'to eat'

Family	*Language*	*Attestation*	*Gloss*
Korean	MK[10]	capso(p)-si-	eat (honorific)
Korean	K	capswusi-	eat (honorific)
MT[11]	Ewenki	jeb-	eat
MT	Ewen	jeb-	eat
MT	Solon	jeb-	eat
MT	Oroch	jepte-	eat
MT	Udehe	jepte-	eat
MT	Ulchi	jep-	eat
MT	Orok	dep	eat
MT	Nanai	jeb-	eat
MT	Manchu	je-	eat
Turkic[12]	Turkish	ye-	eat
Turkic	Turkmen	iy-	eat
Turkic	Azeri	ye-	eat
Turkic	OT	ye-	eat
Turkic	Kirghiz	je-	eat
Turkic	Uzbek	ye-	eat
Turkic	Kazakh	je-	eat
Turkic	Altai	d´i-	eat
Turkic	Tuvinian	ci-	eat
Turkic	Tofalar	ci-	eat
Turkic	Yakut	sie-	eat
Turkic	Chuvash	_i-	eat

Table 15.9 Japonic terms for 'ear of grain' PJ *pwo 1.3a

Group	*Language*	*Attestation*
Japanese	OJ	pwò
Japanese	MJ	fò
Japanese	J	hó
Japanese	KY	hóó[13]/hòó
Japanese	KA	hó[14]
Ryukyuan	SH	hùù
Ryukyuan	NJ	p'ùú

Commentary 15.8 and 15.9

These forms should go back to pre-PJ *paCu or *puCa. Benedict would probably compare the reconstruction *puCa with PAN *bèɣat´ 'rice' despite

the phonetic problems. If *-Cu* in *paCu is a suffix, the word is better compared to Proto-South-Bahnaric *phèè 'rice' (Efimov 1990: 140).

Table 15.10 Japonic terms for 'ricefield' PJ *ta 1.3a

Group	*Language*	*Attestation*
Japanese	OJ	tà
Japanese	MJ	tà
Japanese	J	tá
Japanese	KY	tàá
Japanese	KA	tà
Ryukyuan	SH	tàà
Ryukyuan	NJ	t'àá

Table 15.11 Altaic terms for 'field', 'plain'

Family	*Language*	*Attestation*	*Gloss*
Korean	MK	tùlúh-	field
Korean	K	tul	field
Korean	HK[15]	twulwu(i)	field
Mongolic[16]	WM	tala	field, plain
Mongolic	Khalkha	tal	field, plain
Mongolic	Monguor	talaa	field, plain
Mongolic	Dahur	tal	field, plain
Turkic	OT	tala	field, plain
Turkic	Uzbek	dala[17]	field
Turkic	Uighur	dala[17]	field
Turkic	Yakut	taala	field, plain

Commentary 15.10 and 15.11

This word may go back to pre-PJ *tara, on the basis of Whitman's law of medial *-r- loss (Whitman 1990). If so, it has the cognates shown in Table 15.11 in the Altaic languages.

Table 15.12 Japonic terms for 'rice bran' PJ *nuka ?2.3

Group	*Language*	*Attestation*
Japanese	OJ	(nuka)[18]
Japanese	MJ	núkà
Japanese	J	nùká
Japanese	KY	núkà
Japanese	KA	núkà
Ryukyuan	SH	nùkà
Ryukyuan	NJ	nùkàá

Commentary 15.12

Martin (1987: 502) suggests internal etymology from *nuka- kapa 'remove skin' with haplology, but this seems questionable to me. There are no obvious internal or external etymologies.

Table 15.13 Japonic terms for 'flour, powder' PJ *kwo 1.3a

Group	*Language*	*Attestation*
Japanese	OJ	kwo
Japanese	MJ	kò
Japanese	J	kó
Japanese	KY	kòó
Japanese	KA	kò
Ryukyuan	SH	kùù
Ryukyuan	NJ	hùú

Commentary 15.13 and 15.14

Similarly to PJ *ta 1.3a 'ricefield' < pre-PJ *tara, PJ *kwo 1.3a 'flour', 'powder' may go back to pre-PJ *kaCu/*kuCa < *karu/*kura, as suggested by Whitman (1985: 145–6). Then it is likely to be related to the PK *kòlòl/*kòlòGòl 'flour', 'powder' (Whitman 1985: 147) and to Manchu halu 'fine flour' (Table 15.14).

Table 15.14 Altaic terms for 'flour, powder'

Family	*Language*	*Attestation*	*Gloss*
Korean	MK	kòlò/kòlG-	flour, powder
Korean	K	kalwu	flour, powder
Korean	HK	kalki	flour, powder
Korean	CK	kulu(l)	flour, powder
MT	Manchu	halu[19]	(fine) flour

In spite of the fact that the Manchu word is isolated in MT, the initial *h-* in Manchu excludes the possibility of it being a recent loanword from Korean *kalwu*. A possibility of an old loan from Korean is unlikely, too, given the specific nature of MK (and probably PK as well) vocalism of the word. Therefore, PJ *kwo 1.3a 'flour', 'powder' seems to have a reliable Altaic etymology.

Table 15.15 Japonic forms for 'starch, rice glue' PJ *n̲o̲ri 2.3

Group	*Language*	*Attestation*
Japanese	OJ	no̲ri
Japanese	MJ	nòrì
Japanese	J	nòrí
Japanese	KY	nórì
Japanese	KA	nórì
Ryukyuan	SH	nùì
Ryukyuan	NJ	nùí

Commentary 15.15 and 15.16

Since there is a restriction on combination of PJ *o̲ and *u within the limits of a bimoraic word, it leaves us only two choices for possible pre-PJ forms: *no̲ri or *no̲ro̲-Ci. The Korean etymology in Table 15.16 supports pre-PJ *no̲ro̲-Ci rather than *no̲ri.

Table 15.16 Korean terms for 'yeast, leaven, malt'

Language	*Attestation*	*Gloss*
MK	nwùlwúk	yeast, leaven, malt[20]
K	nwulwuk	yeast, leaven, malt
CK	nwulwuk	yeast, leaven, malt

No Altaic etymologies outside Korean are known to me.

CONCLUSION

The statistics for these ten basic terms are as follows:

- Three out of ten words (*momi 2.1 'unhulled rice', *do̲na-Ci 2.1 'hulled rice', *nuka ?2.3 'rice bran') have no external etymologies.
- Four terms: *ipi 2.3 'cooked rice', *ta 1.3a 'ricefield', *kwo 1.3a 'flour', *no̲ri 2.3 'starch', 'rice glue' have an Altaic origin; the first three have parallels in Korean as well as in other Altaic languages, meanwhile the last one is supported exclusively by a Korean etymology.
- The remaining three words: *(z)ina-Ci/*(h)ina-Ci 2.4 'riceplant', *ko̲ma-Ci 2.3 '(hulled) rice', *pwo 1.3a 'ear of grain' can be assigned tentative Austroasiatic etymologies.

None of these words, to the best of my knowledge, has any parallels in Austronesian, Tai, or Miao-Yao.

These last three etymologies suggest that the Yayoi people were an Austroasiatic people. This agrees with a Yayoi homeland located in Southeast China,[21] which is often believed to have been populated in ancient times by Austroasiatic peoples: modern Min dialects of Chinese located there in the present have an Austroasiatic substratum (Norman 1988: 18). The existence of Altaic etymologies for certain other rice agriculture terms suggests a situation when the indigenous population was subjected to linguistic assimilation, probably during the Kofun period. Therefore, I suggest a slightly different scenario for Japanese ethno- and glottogenesis than that proposed by Ledyard (1975), though I agree with him on the main point: a Puye conquest of Japan in the fourth century AD. However, the above evidence suggests that mounted invaders from the mainland subjugated the native Yayoi population once and for all, assimilating them linguistically to such an extent that even

some aboriginal terminology in the typical native area of rice agriculture was replaced. This leaves no place for any kind of Wa 'nativist rebellion' (Ledyard 1975: 251–4). By the end of the fifth century, the Wa people probably were speaking the Altaic language of their conquerors, and it is only in such small pockets as rice agriculture terminology, that we can still find traces of their original language.

It may be that the ethnic name of the Yayoi people, Wa 倭, recorded in the 'Weizhi woren zhuan' (OC *?wa), coincides with the ethnic name of a group of Austroasiatic peoples still living in Yunnan, Burma and Thailand (*wa, la-wa, ka-wa* etc.). I do not see any possible Austronesian connections for this ethnic name.

Finally, it is not my intention to exclude possible prehistoric Austronesian inhabitants of Japan. It may very well be that there were some in Japan. However, it is probably in the area of Japanese nautical terminology that their traces can be discerned. As far as rice agriculture is concerned, there is no linguistic evidence for Austronesian presence in Japan.

NOTES

1 Japanese proper names and titles of books are given in the Hepburn romanization. Japanese linguistic data are rendered in the Yale system of romanization. All Old Chinese data are in romanization proposed by Baxter 1992. For Korean Yale romanization is used throughout. Other Altaic languages are rendered in usual Altaistic transcription with few modifications: c=[č], j=[ǰ]. Other languages follow the transcription used in the source.
2 Numerical notations represent different PJ noun accent classes (H – high pitch, L – low pitch, X. – number of moras in a word): 1.1: H-H, 1.2 H-L, 1.3a L-L, 1.3b L-H, 2.1 HH-H, 2.2a HH-L, 2.2b: HL-L, 2.3: LL-L, 2.4: LH-H, 2.5: LH-L, given here according to Martin (1987).
3 For Japonic languages, OJ data are cited according to Omodaka *et al.* (1967), MJ according to Mochizuki (1974) and Ohno *et al.* ([1974] 1990), KY and KA mostly according to Hirayama ([1960] 1989), SH according to Iwabuchi *et al.* (1976), and NJ according to Nakasone (1983). PJ reconstructions mostly agree with Martin (1987), with minor differences.
4 The following diacritics are used as pitch notations: ` – low pitch, ´ – high pitch, ^ – falling pitch.
5 Replaced by a Chinese loan *méé* 'rice plant' < MC *mej* '(hulled) rice'.
6 No phonetic attestation in OJ (Omodaka *et al.* 1967: 747).
7 Attestations only in compounds (Hirayama [1960] 1989: 844).
8 No known Ryukyuan attestations.
9 Irregular accent pattern in J.
10 Middle Korean data are cited according to Yu ([1964] 1987).
11 Most of Manchu-Tungus data are cited according to Tsintsius (1975–7).
12 Turkic data follow Egorov (1964), Nadeliaev *et al.* (1969) and Sevortian (1974–89).
13 Irregular accent pattern in KY.
14 Irregular accent pattern in KA.
15 Korean dialect data follow Choy (1978).
16 Mongolic data are cited according to Lessing *et al.* (1960) and Sun *et al.* (1990).
17 Initial d- in Uzbek and Uighur is irregular (t- should be expected).

18 No phonetic attestation in OJ (Omodaka *et al.* 1967: 552).
19 The Manchu word appears in Norman (1978: 124a) and Walravens and Gimm (1978: 319), which is a reverse dictionary of Gabelentz (1864), unavailable to me now. Interestingly, tbe word *halu* appears in Zakharov (1875: 389) only with the meaning 'fine noodles from rice flour'. Since Norman lists the word with the meaning 'fine flour or meal', I wonder whether it can be the same word.
20 For the semantic aspect of comparing 'rice glue' with 'malt' consider K *nwulwuk-mith* (*mith* 'base') 'malt made of glutinuous rice'.
21 However, see Hudson (1994) for an alternative view.

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16 *Rice in Southeast Asia: a regional interdisciplinary approach*

ILIA PEJROS AND VICTOR SHNIRELMAN

INTRODUCTION

The question of whether rice was domesticated only once and in one particular area or simultaneously in several regions remote from one another has been a point of long and lively discussion. Some botanists believe that rice was originally domesticated in one narrow region and afterwards introduced to other areas in the course of later migrations (Watabe 1985). Barrau (1974: 31), for example, suggested that rice was brought to Southeast Asia by migrants from the north who were already cereal cultivators. Other scholars (Yen 1982; Chang 1989; etc.) adopt a polycentric approach, proposing that rice was domesticated in different parts of a vast region including South China, East India and Southeast Asia. The same two approaches are also popular among archaeologists and prehistorians. Chesnov (1973, 1976) and Sorensen (1972, 1986) believe that rice cultivation spread from a particular area of South China, while others (Bayard 1975; Glover 1979, 1985; Bellwood 1985) support a polycentric view (see also Shnirelman 1989).

This chapter attempts to bring together evidence from different disciplines for the spread of rice cultivation in Southeast Asia. First, we discuss the linguistic prehistory of the region examining the issue of linguo-cultural contacts in the light of the reconstruction proposed by Pejros. Then we present archaeological and ecological data reviewed and analysed by Shnirelman. The hypotheses concerning rice cultivation origins and its dispersal are the result of our joint research.

LINGUISTIC DATA

Five language phyla are recognized in modern Southeast Asia: Austroasiatic, Miao-Yao, Austronesian, Tai-Kadai [Daic] and Sino-Tibetan. The first four perhaps formed an ancient Austric macrofamily (Pejros 1998) with two major branches: the Miao-Austroasiatic (Miao-Yao and Austroasiatic) and the Austro-Tai (Austronesian and Kadai). In some publications the term 'Austric' is applied

to a proposed linguistic macrophylum that includes *only* Austroasiatic and Austronesian (Diffloth 1994; Reid 1994). Data for Proto-Austric are extremely scarce, but, as the daughter proto-languages do not indicate a tropical environment (for Austronesian: Blust 1984–5; for Austroasiatic: Pejros 1998) one can assume that its homeland was located somewhere northward of the tropical zone of eastern Eurasia. The disintegration of Proto-Austric can be very roughly dated to *c.* ninth to eighth millennium BC, while a break-up of the Miao-Austroasiatic and Austro-Tai branches had perhaps begun by the sixth millennium BC. However all hypotheses about the initial localization of Proto-Austric and its subsequent split, are still highly speculative, as is the suggested dating. The linguistic dates here are suggested by Pejros (1998) and are based on improved glottochronological method (Starostin 1989).

There are reasons to believe that by the end of the fifth millennium BC the Austroasiatic family was represented by several branches, including Munda and Mon-Khmer, which had already split from each other. The proto-languages for these two groups could be independently reconstructed but this has not yet been done. By the end of the fourth millennium BC Proto-Mon-Khmer began in its turn to break up, giving rise to many language groups such as Khmer, Bahnaric, Viet-Muong among others. Most of these groups have been in long and close contact with each other in the Southeast Asian cultural zone. Therefore they could have borrowed terms from each other rather than inheriting them directly from their ancestral proto-language. Without a detailed reconstruction of Proto-Mon-Khmer and Proto-Austroasiatic it is difficult to distinguish between these two categories of lexical items. It is quite possible, however, that several terms for rice, found in both Proto-Munda and Proto-Mon-Khmer, are of common Austroasiatic origin and can be reconstructed for Proto-Austroasiatic. In Khmer, for example, one can find the following words of Proto-Mon-Khmer or Proto-Austroasiatic origin; *sru:w* paddy, *khəsa:j* kind of rice, *ʔəŋko:r* paddy. Terms of the same origin are known in other languages of the family, but we give the Khmer forms simply because the phonological reconstruction of Proto-Mon-Khmer is not well established. Further ancient terms for rice can be identified in the other Mon-Khmer languages.

Using linguistic evidence Pejros (1989, 1998) has argued that the Austroasiatic homeland was located in mainland eastern Eurasia in an area with a non-tropical climate: the sub-tropics or the mountainous region. During the climatic optimum of the Middle Holocene, when average annual temperatures were higher by 2–4 degrees Celsius (Bellwood 1978; Sorensen 1986: 271), the tropical zone included territories 200–400 km north of its modern border. This means that the Middle Holocene tropical zone would have included Fujian and most of Zhejiang while the wet sub-tropical zone would have included Jiansu and territories up to Shandong. As in Proto-Austroasiatic we do not have evidence for a coastal environment, and it is therefore possible to assume that the Proto-Austroasiatic homeland was located somewhere near the Middle Yangzi River Valley.

The prehistory of the Austronesian languages has been the focus of much more detailed investigation than the prehistory of other Southeast Asian language families. The main features of Proto-Austronesian, such as the phonological reconstruction, are already known. However there is still no generally accepted genetic classification of the family. The Austronesian classification of Blust (1980) is not purely genetic. Blust defines Western-Malayo-Polynesian not as a genetic unit with its own specific innovations, but rather as a residual group which did not undergo changes characteristic of the languages of other groups. The genetic nature of the primary split between Formosan and other languages is not properly motivated, and thus is also questionable. It therefore cannot be always reliably used for the stratification of different features found in the languages nor for justifying cultural reconstructions. An additional difficulty is the intensive contact between speakers of different Austronesian languages traceable back to the remote past and which has resulted in many shared features observed in several cultural zones.

According to Blust (1984–5) the break-up of Proto-Austronesian started in the fourth millennium BC. He also argues that the Proto-Austronesian homeland may be located in Taiwan, or elsewhere in adjacent mainland China. Sirk (1987) tends to support a mainland location. Additional evidence for this is in Austronesian contacts with the Sino-Tibetan languages (Benedict 1975; Pejros and Starostin 1984) which until the very recent past could have occurred only on the mainland. Blust and Sirk agree that the Proto-Austronesian homeland had to be situated in a sub-tropical rather than a tropical zone. The linguistic arguments for this assumption are based on the Proto-Austronesian terms for wet sub-tropical vegetation. However in the Middle Holocene, as has already been mentioned, the climate was warmer and the homeland could have been located further to the north, probably in Jiangsu or even in Shandong.

There are three main Austronesian roots designating rice which may be of ancient origin: **pag'aj, *beRas* and **imaj*. Some linguists (Blust 1976; Sirk 1987) believe that the reconstructed terms for rice belong to the lexicon of the proto-language, and that Proto-Austronesian speakers did in fact cultivate it. Dyen (1971) believes that there were no terms for rice in Proto-Austronesian and that the Proto-Austronesian speakers were not familiar with the plant. He argues that only later divergent Austronesian groups began to cultivate rice, a position recently supported by Mahdi (1994: 434–41). Although Proto-Austronesian knowledge of rice seems to be questionable, Austronesian speakers certainly began to cultivate rice rather early.

It is very difficult to judge what happened to Kadai and Miao-Yao languages in the fifth to fourth millennia BC, since Proto-Kadai and Proto-Miao-Yao reconstructions reflect much later time-periods. It is however worth mentioning that no term for rice can be reconstructed for the Proto-Kadai period; while the word **mblai*^{A} paddy is found in Proto-Miao-Yao and thus is dated to the beginning of the second millennium BC at the latest.

Pejros (1996), using purely linguistic data, argues that the homeland of Proto-Sino-Tibetan has perhaps to be located in the Sub-Himalayan area

rather than in East or Southeast Asia (see also van Driem, Ch. 2, this volume). The break-up of Proto-Sino-Tibetan can be dated back to the fourth millennium BC. This brought a few daughter languages to the east, first to the areas of modern South China and only later to Northern China. Proto-Sino-Tibetan, as it is reconstructed by Pejros and Starostin (1996), had several terms for rice: **mʌj^h^* or **mʌlH*, **bria(s)*, and possibly a few more.

There is strong evidence for linguistic and thus cultural contacts between speakers of the three major regional language phyla (Austroasiatic, Austronesian and Sino-Tibetan). It is highly probable that contacts between Austroasiatic and Sino-Tibetan languages started in the proto-language period, but were later interrupted presumably as a result of migrations of the Sino-Tibetan-speakers. Since the list of loanwords does not include cultural terms, there is no reason to suggest that either of those two groups was in a dominant position in the contact situation (Pejros 1989, 1998).

There are two major possibilities regarding the nature of contacts between Austronesian and Sino-Tibetan languages. Benedict (1967, 1975) argues that Proto-Austronesian and its ancestor were dominant in the contact situation, reflected in the Austronesian (or Austro-Tai) impact on the Sino-Tibetan-speakers. In contrast, Pejros (1989) supports the idea that the majority of the loanwords are of Sino-Tibetan origin, i.e. that they were borrowed by the Austronesian speakers (Pejros and Starostin 1984). His argument is based on the hypothesis that proto-Sino-Tibetan was a member of the ancient Sino-Caucasian macrofamily of languages (Starostin 1984), and thus that Sino-Tibetan words of common Sino-Caucasian origin could not have been borrowed from an Austronesian source.

The linguistic data allow us to postulate that by the fifth to fourth millennia BC rice was known in the southeastern parts of Eurasia. Proto-Sino-Tibetan and Proto-Austroasiatic speakers were among the groups who cultivated rice and other cereals. This is suggested by the existence of several terms for rice in both proto-languages. Since there is evidence of cultural contact between speakers of these proto-languages, we cannot disregard the possibility that the proto-Sino-Tibetan speakers, who migrated to the east from their former territory, borrowed rice cultivation technology from an unknown eastern group. They could have named the plant with an old Sino-Caucasian word associated primarily with another cereal (millet?). At the same time there is no indication that the knowledge of rice came to the Sino-Tibetan speakers via or from their remote Austroasiatic neighbours. This allows us to put forward the hypothesis that the speakers of these two language families developed the knowledge of rice cultivation either independently or borrowed it also independently from other local groups of unknown linguistic affiliation.

The origins of rice cultivation among ancient Austronesian speakers are much more obscure. We believe that two terms for rice **beRas* and **imaj*, which can be attributed to rather early stages of Austronesian linguistic evolution (Proto-Austronesian or more likely a period just after the break-up of the proto-language), were borrowed from a Sino-Tibetan source along with many

other cultural terms. A third Austronesian word for rice, **pag'aj*, was definitely not borrowed from Sino-Tibetan, but resembles the form **mblaɨ*[A] reconstructed by Pejros for Proto-Miao-Yao (Yakhontov personal communication) or is perhaps associated with Proto-Mon-Khmer **Cəsaj* > Khmer *khəsa:j*. There are two alternative explanations for these similarities. Either they represent an ancient, probably Proto-Austric, term for rice, or alternatively the word was borrowed from one language group into another after the break-up of the proto-language. The current state of knowledge on the relationships between the ancient languages of Southeast Asia is insufficient to determine the direction of such a borrowing. Nevertheless, in our view, a borrowing from Mon-Khmer into Austronesian is more probable, because after factoring out Sino-Tibetan loans, the word **pag'aj* remains isolated in Austronesian, while Proto-Mon-Khmer reveals a whole set of words associated with rice cultivation. This set indicates a sophisticated level of rice cultivation among Proto-Mon-Khmer speakers, which is not found in Proto-Austronesian.

ARCHAEOLOGICAL INTERPRETATIONS

It is well established that rice was cultivated on the coast of Northern Zhejiang (Luo-jia-jiao, He-mu-tu) in the sixth to fifth millennia BC (Chang 1989: 411). Some of these local settlements, which have been studied in detail, represent well-advanced cultures whose origin remains unknown. Originally only the *Oryza indica* sub-species was cultivated there, followed later by *Oryza japonica*. Recently, evidence of rice-cultivation dated to the seventh to sixth millennia BC (Yan 1991) has been discovered in the Middle Yangzi River Valley (Pearson and Underhill 1987: 810). There have also been some discoveries in the Middle Ganga Valley (see Shnirelman 1989: 116–17 for the discussion of their meaning and dating) (Figures 16.1 and 16.2). Together, these findings have given rise to an hypothesis that rice was domesticated inland and was brought to the coastal areas only later. This corresponds fairly well with the hypothesis of a non-Austronesian origin for rice terms.

We are quite convinced that the spread of rice through the Southeast Asian islands was connected with the southward migrations of the early Austronesians, i.e. speakers of ancient Austronesian languages. The earliest rice is dated in Taiwan to the second half of the third millennium BC (Chang 1989; cf., however, Spriggs 1989 for criticism of these dates). In Kalimantan the dates for rice grain in shred are given as 3850±260 BP (Datan and Bellwood 1991: 390). In northeastern Luzon (Philippines) rice has been discovered at a site dated to the second half of second millennium BC (Snow *et al.* 1986), while rice was introduced to Sulawesi (Indonesia) no later than the middle of the first millennium AD (Glover 1977, 1985). These scant but reliable archaeological data accord well with modern theories on the routes of ancient Austronesian migrations. They also disprove the hypothesis of some botanists (Yen 1982; Chang 1989) that rice cultivation spread from south to north in Southeast Asia.

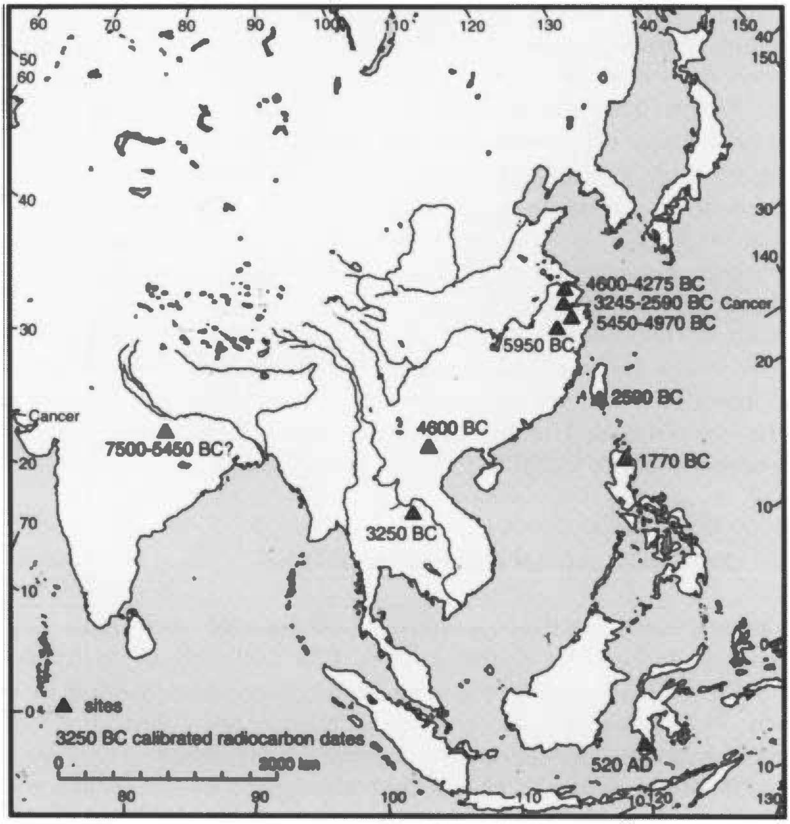

Figure 16.1 Earliest traces of *O. indica* cultivation

It is important to note that the two sub-species of rice – *Oryza indica* and *Oryza japonica* – were brought to Taiwan at roughly the same time. The former was cultivated at the southern tip of the island, while the latter was found on its northern side. This could be explained in different ways. Either the two sub-species were brought by two different groups of migrants, or differences in climate (tropical to the south and sub-tropical to the north) determined the location of the sub-species. The early Austronesians were presumably familiar with both sub-species, but *O. japonica* was lost in the course of migrations to the tropical islands further south.

The *O. japonica* sub-species could have been domesticated somewhere in the mountainous areas of Yunnan or Western Sichuan (Chang 1983) or, perhaps, in areas further west whence it spread eastward along the Yangzi River Valley. The wide dispersal of this sub-species in East Asia took place in the second half of the fourth millennium BC to the first half of the third millennium BC. The domestication of *O. japonica* may thus have occurred in an area which overlapped with the proposed Sino-Tibetan homeland. In

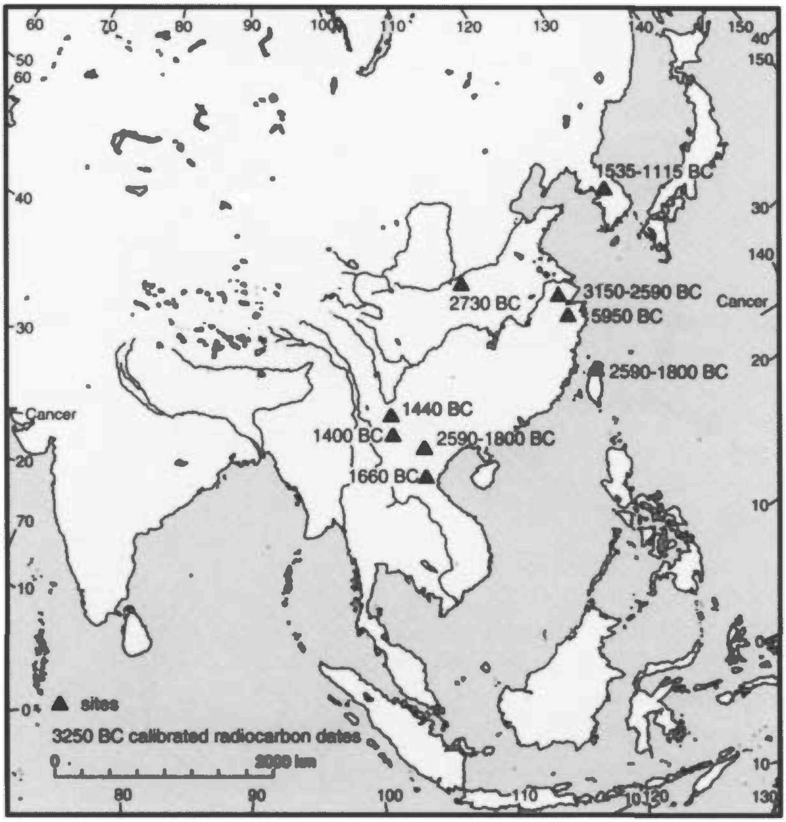

Figure 16.2 Earliest traces of *O. japonica* cultivation

this case it would seem reasonable to assume that Proto-Sino-Tibetan-speakers were among the groups that selected this particular sub-species of rice. Alternatively, they might have gained the relevant knowledge from another local group. The initial development of *O. japonica* could have occurred at the end of the climatic optimum period when the onset of frosts would have encouraged selection of more frost-resistant sub-species such as *O. japonica*.

The eastward migration of the ancient Sino-Tibetan speakers brought the sub-species to Eastern Asia (Figures 16.3 and 16.4). In the course of this migration the Sino-Tibetans presumably encountered the early Austronesians, who could have borrowed *O. japonica* together with its Sino-Tibetan name. It is likely that the early Sino-Tibetan speakers originally cultivated both sub-species of rice, as the Dravidians had borrowed rice-growing technology from a Sino-Tibetan group by the end of the third millennium BC (Pejros and Shnirelman 1992). The Central-South Dravidian word for rice **vari* (Burrow and Emeneau 1984: N.5265) is one of a few Sino-Tibetan borrowings (Pejros 1984). Dravidian–Sino-Tibetan comparisons can be interpreted only as a trace

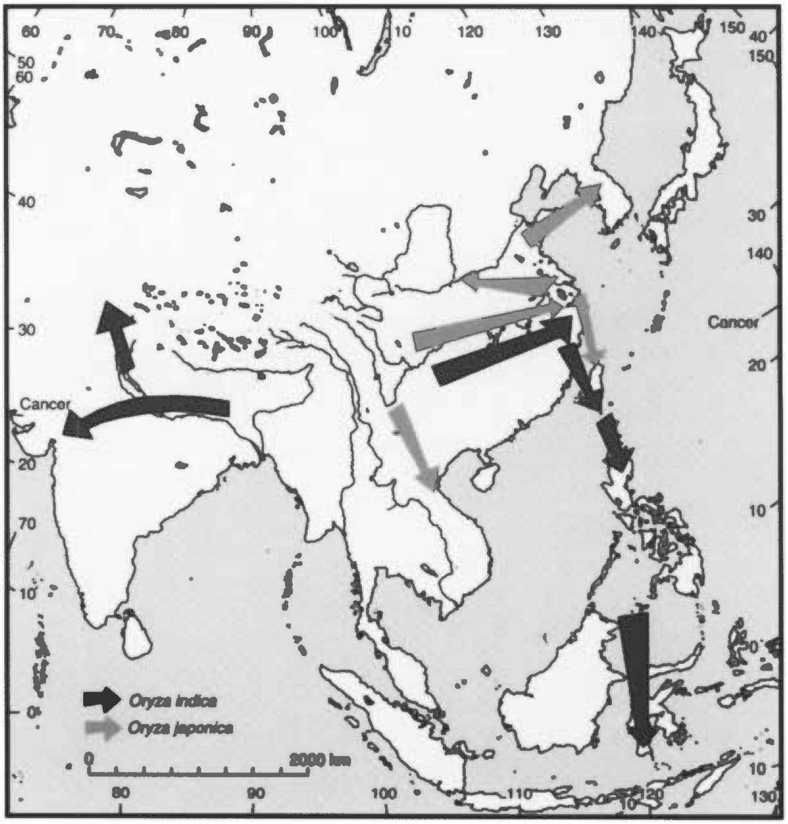

Figure 16.3 Dispersal routes of rice cultivation

of a contact, but it is not yet clear where this contact occurred. The rice which Dravidians cultivated resembled *O. indica.*

If we suppose that early pottery-making cultures, which spread throughout the Southeast Asian islands in the Neolithic, can be associated with the Austronesians (Spriggs 1989), then on the basis of recent archaeological data we can affirm that at least some of these groups cultivated rice. The northernmost of these groups (in the north of Formosa) cultivated *O. japonica* while all the others cultivated only *O. indica.* If this supposition is correct, it suggests that these migrations started after Austronesian contact with the Sino-Tibetan migrants had taken place.

It should be stressed that the appearance of *O. japonica* is by no means an indication simply of Sino-Tibetan migration. In areas such as northern Vietnam, Korea and Japan the spread of this sub-species was doubtless connected with other cultural groups. We hypothesize, however, that ancient Sino-Tibetan-speakers made the initial contribution to the wide dispersal of *O. japonica* and that early Austronesians were among the recipients.

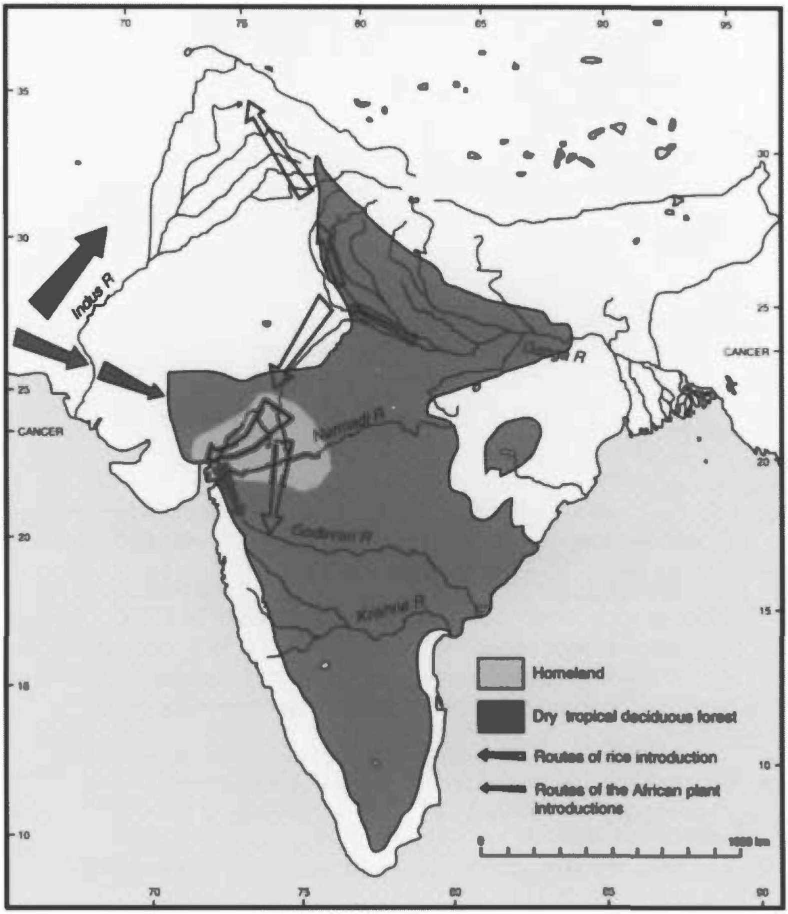

Figure 16.4 Introduction of rice into India

The origins of the second sub-species of rice – *O. indica* – are connected with the groups which inhabited Southern China, Southeast Asia and Eastern India. At this stage it is not clear what languages they spoke, but they were definitely not of Austronesian stock.

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17 *Linguistic data on transmission of Southeast Asian cultigens to India and Sri Lanka*

WARUNO MAHDI

INTRODUCTION

Ever since Vavilov (1927: 417, 425) included Java, Sumatra, and other insular Asian centres among places of origin of domesticated plants, a series of publications have appeared indicating an area encompassing Indochina and South China in the west, and Western Melanesia in the east, as one of the fundamental centres of origin of cultivated plants (Yen 1980). The flooding of the Sunda and Sahul Shelfs (connecting islands of West Indonesia with Indochina, and New Guinea with Australia respectively) between 12,000 and 8000 BC must have led to a concentration of population in Southeast Asia and West Melanesia, in which people gradually retreating from the flooded lowlands met highlanders descending the mountain valleys as a result of the gradual expansion of the continental population. One consequence of this process was possibly an early recourse to more intensive methods of exploitation of food resources, domestication of food plants and animals. Indeed, first signs of apparent plant domestication appear in the course of the following two to four millennia in Indochina and in New Guinea.

Views on South China–Indochina–New Guinea as centre of origin of domesticated plants were substantiated at a relatively early time. For example

alocasia (*Alocasia microrhiza* Schott.)
areca or betel nut (*Areca catechu* L)
banana (*Musa sapientum* L)
betel pepper (*Piper betle* L.)
black pepper (*P. nigrum* L.)
breadfruit (*Artocarpus incisa* L. =*A. communis* G.Forst. = *A. altilis* (Park.) Fosberg)
coconut (*Cocos nucifera* L.)
greater yam (*Dioscorea alata* L.)
jackfruit (*A. integrifolia* L. = *A. heterophyllus* Lam.)
lemon (*C. limon* (L.) Burm.f.)

lesser yam (*D. esculenta* Burk.)
lime (*Citrus aurantifolia* (Christm.) Swingle)
paper mulberry (*Brousonettia papyrifera* Vent.)
rice (*Oryza sativa* L.)
sago (*Metroxylon rumphii* Mart. = *M. sagu* Rottb.)
sugarcane (*Saccharum officinarum* L.), etc.
taro (*Colocasia esculenta* Schott.)

(see Barrau 1963; Purseglove 1968: 658–90, 1972: 567–84; Li 1970; Yen 1977; Bellwood 1976: 154–6)

Not all these cultigens are equally relevant and this chapter discusses those for which either linguistic or historiographic data exist, with implications for botanical observations on their origins.

Methods of historical and comparative linguistics can contribute greatly to clarification of problems of transmission of elements of culture, which also include knowledge and cultivation of various plants. They permit evaluating echoes of past processes, fossilized in the vocabulary of languages like artefacts from archaeological excavations. However, such linguistic evaluations are just as susceptible to misinterpretations as are archaeological ones. This chapter will offer some case studies in which implications of the evaluation of linguistic data are placed side by side with archaeological, historical, botanical and taxonomic data.

TRANSCRIPTIONS

Sanskrit, Arabic, Hebrew, and Greek glosses are given in conventional latinizations for these languages, it not always being possible or desirable to replace them by explicit IPA transcriptions. Thus, for instance, Arabic ǧ was pronounced /g/ in some dialects, /j/ in others. It is not always apparent which dialect was involved, and what the pronunciation was in that dialect at that time, or even whether the time can be narrowed down closely enough to be that specific about its contemporaneous phonology. For Chinese, all glosses are given in the standard Pinyin latinization with indication of the tones.

Modified IPA transcription is used wherever possible for comparative lexical data from Austronesian languages.[1] However, for Malagasy the standard orthographic rendering is used instead,[2] with additional indication of the stressed syllable, and for Paiwan I use the spelling of Ferrell (1982).[3] For Austroasiatic (except Vietnamese),[4] Dravidian, and Indo-Aryan languages I have relied on the transcription used in the sources. For reconstructed Austronesian protoforms (marked like all reconstructs by an asterisk before the item), the transcription of Dyen (1971),[5] simplified in that the indices marking hypothetical distinctions have been deleted, is used with the following modifications:

- *y and *w have been replaced by *i and *u respectively in agreement with Dahl (1977) except in relatively recent protoforms which appeared

after desyllabification of the original high vowels in the corresponding positions (see also Mahdi 1988: 90–101, 1994a: 208–9 n. 41)

- *b is split into *b and *B following Prentice (1974) and Nothofer (1975)
- the 'laryngeals' *q, *S, *ʔ, and *H, the latter including also *h which I do not distinguish as a separate proto-phoneme, are reconstructed in accordance with Zorc (1982)
- the opposition of *d and *D is redefined as in Mahdi (1988: 72–8, 1994a:170).

Furthermore, in agreement with Wolff (1974, 1982), I do not include *c, *r, *T and *z among the original phonemes of Proto-Austronesian, but regard them either as features of various Austronesian meso-languages (lower-order, intermediate proto-languages), or as phantom proto-phonemes reconstructed from sound correspondences from cognate comparisons involving loanwords.

Optional parts of a protoform, e.g. when an affix is not represented in all the reflexes, will be enclosed in round parentheses, so that *(a)bc means '*bc and/or *abc'. Uncertain and alternative potential items in the reconstruction will be placed in square brackets, whereby *ab[c] means 'probably *abc, but the *c is uncertain', whereas *ab[cd] means 'either *abc or *abd'. Furthermore, *a[ ]b means '*a...b with perhaps something not yet specifiable in the place of the dots'. The part of a reflex or cognate which is relevant for the reconstruction or comparison will be separated from irrelevant parts (affixes, other components of compounds) by a hyphen, as e.g. in English *hund-red*, Latin *cent-um*, or in English *yard*, German *Gart-en*, Russian *o-gorod*, Latin *hort-us*. I shall furthermore use ø to denote the 'zero or non-phoneme'.

THE BOTTLE-GOURD

One preliminary example will perhaps not be out of place, to demonstrate the possible misinterpretations that may occur. Przyluski (1926: 31–3) claimed that Sanskrit alābu ~ alābū ~ lābū 'bottle-gourd (*Lagenaria siceraria* (Molina) Standl. = *L. vulgaris* Seringe = *L. leucantha* (Duch.) Rusby)' was borrowed from Austroasiatic (see also Mayrhofer 1953–6: 55, 1967: 99; Turner 1966: 31). Przyluski confused Austroasiatic with Austronesian languages, because Khmer *lbow* was the only Austroasiatic data in the evidence (otherwise from Malay, Toba or Karo Batak, Malagasy, Javanese, Sundanese, Madurese). The actual Mon-Khmer (and thus Austroasiatic) evidence suggests another protoform unrelated to either Austronesian or Sanskrit:

'bottle-gourd'		*pūr
East Austroasiatic:	Kha	păr
	Rongao	puɣr
	Bahnar	puol
	Vietnamese	bəw_{C1} < *pəuʔ

Cognates of alābū in Austroasiatic are rare, and not attested at all for either Old Mon (Shorto 1971) or Old Khmer (Jenner 1981–2; Pou 1993). However, cognates are widespread in Austronesian languages of Indonesia. In the Philippines, cognates occur only in the extreme south, whereas in Indochina none has been reported. Most Austronesian cognates answer to an effective *laBu as a protoform, a lesser number to the apparent doublets *ta[bB]u and *walu[q], the latter being related to *laBu by metathesis and shift of the bilabial obstruent to a glide (see also de Clercq 1909: #1973).

'bottle-gourd'	*walu[q]		*walu[q]			*walu[q]
Java:§	Sundanese	_waluh_	Old Javanese	_waluh_	Balinese	_waluh_
North Sulawesi:					Gorontalo	_walu_
Central Maluku:					Buru	_walu_

		*laBu		*laBu		*laBu
Sumatra:§	Simalur	_lafu_	Mentawai	_labu_	Acheh	_labu_
	Mandailing	_labu_	Malay	_labu_	Rejang	_labaw_
Java:§			Sundanese	_labu_	Madurese	_labu(h)_
Sulawesi:	Buginese	_lawo_	Makassarese	_lau_	Mamuju	_labu_
	Bare'e	_lawu_	Kaidipan	_labu_	Tombulu	_labu_
South Philippines:			Tirurai	_labu?_	Maranao	_labo-gandaŋ_
East Central Maluku:			Asilulu	_lapu_	Nusalaut	_lapu_

		*ta[bB]u			*ta[bB]u	*ta[bB]u
Sumatra:§					Nias	tawu†
	Karo	_tabu_	Toba	_tabu_	Lampung	tabu
Madagascar:					Mérina	távo
Sulawesi:	Tomini	_tabo_				
Lesser Sundas			Haruku	_tahu-r_	Fordata	tavu-l
and Maluku:			Manggarai	_tawu_	Babar	tawa-l melai

† hanging bamboo receptacle, § with adjacent smaller islands

Nevertheless, there is no reason to consider the Sanskrit forms as borrowings from Austronesian. The bottle-gourd originates from Africa (Purseglove 1968: 676), and the Austronesian forms must ultimately derive from Sanskrit. In insular Southeast Asia there were two main protohistoric trade routes along which loanwords were transmitted, particularly by Malay-speaking seafarers: the first, beginning from the second century BC, led from Sumatra and the Malayan Peninsula eastwards through the Sangir and North Sulawesi area to the Moluccas, was the clove trade route; the second, beginning from the third or fourth century AD, passed through the Java Sea, the Strait of Makassar, and the Philippines as alternative route to China during Funan maritime control over the South China Sea (Mahdi 1994a). The distribution of forms

for 'bottle-gourd' follows the former, suggesting that the relationship with early loans from Sanskrit. This is also confirmed by the Madurese cognate, because if it were a later borrowing via Malay (*c.* fourth century AD onwards) one would have expected **labhu(h). The reconstructed *laBu appears to be the oldest of the three Austronesian quasi-reconstructions, probably deriving from a Sanskrit borrowing into Malay, and disseminated by Malay-speakers throughout the Archipelago. The doublet *walu[q] probably first emerged in Old Javanese through metathesis either of the Sanskrit form (Zoetmulder 1982: 2186) or of the Malay form. *ta[bB]u is somewhat enigmatic, because the *l > *t shift it suggests is rather unusual in Austronesian historical linguistics, where the alternation *l/*t more often than not derives from alternative prefixes to a common root. One may speculate that the shift took place before borrowing into Austronesian. Indeed, a process such as

l > ḷ > ḍ > ṭ

is imaginable for Indic languages. Perhaps, therefore, *tabu represents an even older borrowing than *labu, mediated by India-based Austronesians during the period before involvement of Malay-speakers in shipping across the Bay of Bengal. Przyluski (1926: 31–2) suggested the existence of a direct relationship between the Austronesian doublet formation *ta[bB]u/*laBu and Sanskrit *tumba* 'bottle-gourd'/(a)lbu 'id.', but this would imply rather extraordinary phonological developments in the t-variant.

THE COCONUT

The coconut is generally considered to have originated in the Southeast Asia and the Southwest Pacific area (see Purseglove 1972: 447–8). This seemed to be confirmed by the discovery of what was seen as a closely related fossilized species (*Cocos zeylandica* Berry), and pollen very similar to that of *C. nucifera* L. was reportedly uncovered in archaeological sites in New Zealand (Purseglove 1972: 449–50). Newer results indicate, however, that closely related species apparently existed along the entire southern coast of Tethys Sea before the northward drifts of the Indian and Sahul (New Guinea–Australian) shelves, and that *Cocos nucifera* was apparently domesticated in insular Southeast Asia and the South Pacific (Harries 1990).

The Austronesian protoform was reconstructed as *niyuR by Dempwolff (1925: 45, 1938: 108 under *nijuɣ), later modified to *ñiuR by Dyen (1953: 13 §56, 55 n. 49). Blust (1978: 16, 28–31, 35, 37, 56–7) has demonstrated that the initial nasal must indeed have been *n as in Dempwolff. Stresemann (1927: 82–3) had assumed the protoform to be *niwəR, to account for numerous reflexes in Central Maluku languages not conforming to either Dempwolff's or Dyen's reconstruction. I have tried to unify the seemingly divergent sets of reflexes, accounting for the apparently 'irregular' palatal ñ- in reflexes noted by Dyen, as well as for reflexes of *ə in cognates observed by Stresemann, and

reconstructed *niəuR. The reflexes with initial ñ- are regular in having ñV for *niV, where *V is any vowel which, when it is a schwa (*ə), assimilates to the preceding *i. The Central and South Maluku reflexes exhibit *ə/*u metathesis. The southern origin of the coconut is confirmed by the circumstance that the protoform is widely represented in Western as well as Eastern Austronesian languages (Kern 1889). Throughout the area such consistency is usually shared only by words referring to so-called 'basic' vocabulary. No cognate forms have been reported so far in languages of Taiwan.

'Coconut'

		*niəuR		*niəuR		*niəuR
Philippines:			Ivatan	*nioy*	Aklanon	niyóg
	Tagalog	*niyog*	Hanunoo	*niyúg*	Isinay	niuh
Sumatra:§	Simalur	*nol*	Malay	*ñiur†*	Krui Lampung	ñiwi†‡
Java:§			Madurese	*ñεyɔr†*	Old Javanese	nyu
Kalimantan:	Ma'anyan	*niuy*	Ngaju	*e-ñoh*	Bintulu	vu-ño
Sulawesi:§	Tomadio	*~nuk*	Talaud	*niukka*	To Bungku	nii
Belau-Marianas:			Belau	*lius*	Chamorro	niyok
Maluku and Lesser Sundas	Soboyo	*nuo*	Kamarian	*niwer*	Bonfia	nuos
:\ P	Sawu	*ñiu*	Roti	*nɔ*	Kai	nuer
South Halmahera-Irian Jaya:			Sawai	*niwe*	Makian	niwi
			Sobei	*niwe*	Minyaifuin	niwi
Micronesia:	Trukese	*nūū*	Marshallese	*ni*	Kusaie	nu
North Papua-New Guinea:			Wogeo	*ni*	Agomes	niu
	Mussau	*niu*	Kaiwa	*niuk*	Numbami	niul
South Papua-New Guinea:	Are	*niura*	Sinaugoro	*niu*		
	Tavara	*neula*	Molima	*niula*	Motu	niu
Solomons:	Arosi	*niu*	Nggela	*niu*	Bugotu	niu
	Sa'a	*niu*	Ulawa	*niu*	Vaturanga	niu
Vanuatu:	Paama	*niu*	Sesake	*niu*	Pangkumu	ni
Loyalties and New Caledonia:			Lifu	*nu*	Xaracu Kanakese	nũ
Rotuma-Fiji:	Rotuma	*niu*	Way Fiji	*niu*	Mbau Fiji	niu
Polynesia:	Uvea	*niu*	Tonga	*niu*	Hawaii	niu

† *niə- > ñi-,
§ with adjacent smaller islands,
‡ *R > i,
all with metathesis *ə/*u

All parts of the plant are used (the kernel liquid, kernel meat, kernel shell, husk, sap of the inflorescence, fibre, entire leaf frond, individual leaflet, leaf-rib, trunk). Moreover, coconut fibre in strong cords played a crucial role in boat building, in the construction of the so-called 'sewn boats'. The emergence of a word for 'coconut', *nārikela*, in the Rāmāyaṇa (see Mahdi 1994a) provides us with the latest date, before which the palm must have been introduced to the South Asian subcontinent. This must have taken place before the first century BC, perhaps even considerably earlier, particularly on the surrounding islands (Sri Lanka, the Maldives). First, it is not clear whether the Rāmāyaṇa in the cited passage reflects contemporaneous conditions, or perhaps recounts an existing tradition of undefined antiquity. Second, Hornell (1945: 173) cites a legend dated 161 BC of a contest between the kings Dutthagamini and Elala whose warriors were armed with trunks of the coconut and the palmyra respectively. In the first century AD the coconut was already appearing on the west coast of India, and by AD 120, the Śaka Uṣavādata began to grant whole coconut plantations of several thousand trees each on the west coast to Brāhmaṇas (Kosambi 1965: 189). Tradition holds that the ancestors of the Tiyyans and Izhuvans inhabiting the Malabar coast introduced the coconut and the toddy palm (*Arenga saccharifera* Labill.) apparently from Sri Lanka. Both names for these people can be traced back to a word for 'island' or an old name for Sri Lanka (Anantakrishna Aiyar 1909: 277). They are closely related to the toddy-palm-cultivating Shannar of the east coast, who are reported by Hornell (1920: 235) to physically resemble Javanese and South Sulawesians. Hornell regarded them and the Izhuvans to ultimately originate from Malayo-Indonesia and specified that the etymology of the name Tiyyan apparently alluded to by Iyer above. It was considered a corruption of *tivan* 'islander', from *tivu* 'island' (Hornell 1945: 173).

The word for 'coconut' in Dravidian languages apparently also indicates a Sri Lankan origin, because it reflects a compound of the words for 'south' and 'fruit' (Tamil *ky*) and thus literally translates as 'southern fruit'.

	#3408†			#3449†	
Tamil	tēṅkāy	'coconut'	Tamil	teṇ	'south'
Malayalam	teṅṅaṅ-kāy	'coconut'	Malayalam	ten	'south'
Kannaḍa	teṅgāy	'coconut'	Kannaḍa	teṅ-ka	'the south'
Telugu	teṅkaya	'coconut'		teṅ-gali	'southwind'
Kota	[ten kāy	'coconut'			

† cognate set number in Burrow and Emeneau (1984)

Sri Lanka is reckoned in Tamil literature as lying to the 'south' of India, a view also reflected in geographical works of the Greeks (Hornell 1945: 173). The Sanskrit word, *nārikela*, was borrowed into Greek as *argellia* (Weber 1873: 147), attested in an AD 545 treatise of Cosmas Indicopleustes:

> Another tree is that which bears the *argellia*, that is the great Indian nut.
>
> (Yule and Burnell 1903: 228)

The coconut was apparently domesticated widely in insular Southeast Asia and the South Pacific, and became known in India not later than the second century BC, having been brought over from Sri Lanka where it must have been introduced even earlier, presumably by Austronesians. It is tempting to consider the possibility that coconuts were introduced into India from the south at the same time as urn burial (Mahdi 1994a).

RICE

The most important crop from the Indochina–New Guinea centre to become spread all over Bangladesh, India, Pakistan, and Sri Lanka, is rice. It originates from a relatively narrow zone along the north of Indochina from Assam to the Gulf of Tonkin and the Pearl River (see e.g. Nakao 1958: 399 Fig. 1; Chang 1976: 144 Fig. 2; Watson 1983: 16 Map 2; Bellwood 1985: 209 Map 7.1). Rice husks were discovered in excavations in Thailand, i.e. in Layer 2 of the Spirit Cave (7000–5600 BC), and in the Banyan Valley Cave (5500–3600 BC), but they represented wild varieties (Yen 1977: 591; see also Higham 1989: 56–7). There is archaeological evidence for systematic cultivation of a close to wild form in Zhejiang by 5000 BC (Bellwood 1985: 209). In India, rice appears at the west coast immediately south of the region occupied by Harappan culture (which had wheats and barleys as cereals) towards the end of the period of that culture (2200–1750 BC) and further at around 1600–1200 BC, and in the south-west of the subcontinent at 900–800 BC (Allchin 1969: 325).

The place of origin of rice indicated above lies entirely within the area of Austroasiatic languages before intrusion of Tibeto-Burman, Daic, and Hmong-Mien languages. The Proto-Austroasiatic form for 'rice' can be reconstructed as *(rV)([nŋ])kouʔ (Mahdi 1994b:436, n. 127), where *V stands for an unidentified vowel. Compare for 'rice (husked)'.[6]

		**(rV)([nŋ])kouʔ*		**(rV)([nŋ])kouʔ*		**(rV)([nŋ])kouʔ*
Munda			Kharia	*rɔm kuʔb†*	Geta'	*rkoʔ*
			Juang	*rum kub†*	Remo	*ruŋku*
	Gutob	*rukug*	Sora	*roŋko*		*ŋku^ʔk*
Mon-Khmer			Old Khmer	*raŋko*	Old Mon	*s-ŋoʔ*
	Tailoi	*enko*	Sué	*raŋkao*	Thèng	*h-ŋɔʔ*
	Amok	*nəku*	Khmu	*rəŋkoʔ*	Lamet	*ngo*
Palaung-Wa			Palaung	*răkáu*	Riang	*koʔ₁*
			Lawa	*rəkoʔ*	Wa	*ŋgáuʔ₁*
Viet-Muong			Vietnamese	*ɣāw*$_{C2}$	< pre-Vietnamese	**ř[]kauʔ*
			Sek	*tŏ-ko*	My-son Muong	*kaw*$_{C2}$
Aslian (Malacca):					Pelus Semang	*hē kåʔ*

Kerau-Ulu Tembeling Sakai	rəkuaʔ -

† (-ub) < -u^ʔb < -uwʔ < -ouʔ

It would therefore be interesting to check more closely whether the Indo-European and Dravidian forms in Bloch (1925: 41–2) do not ultimately derive from Austroasiatic. The author considered with good reason that neither did his hypothetically reconstructable ⋆vrījhi ~ ⋆vrīnjhi represent an authentic Indo-European protoform, nor a similarly reconstructed ⋆arki a Dravidian one.[7]

'rice'	⋆vrī(n)jhi		⋆vrījhi	⋆arki	
Indo-European:		Old Greek	*óryza*	Dravidian:	
Persian	*birinj*	Afghan	*vri e*	Tamil	*arici*
Sanskrit	*vrīhi*	Prakrit	*vīhi*	Kannaḍa	*akki*
Prasun	*wuzī*	Shina	*bṛ̃ú*	Toda	*ašk*
Gurēsī	*brīm*	Kohistānī	*blu*	Malayalam	*arici*
				Tulu	*akki*
Kat(e)i	*wriè*'barley'			Belari	*argi*

Based on the knowledge of the period, Bloch proposed that the etymon should be sought in the region of Babylonia, Bactria or Parthia (Bloch 1925: 46). We now know that rice did not enter Persia and India from those regions, but found its way to them through India and Persia, deriving ultimately from north Indochina. The Indo-Aryan forms listed above cannot be directly derived from the Austroasiatic without stretching phonological credibility beyond reasonable limits. Indeed, the contemporaneity of earliest rice on the Indian west coast with the Late Harappan implies that the word for 'rice' was first borrowed into Dravidian. A putative early Dravidian reflex such as ⋆u-rĩgi would then be imaginable as possible precursor of the Indo-Aryan forms.

Some Dravidian cognates of words with the general meaning 'leaf', sometimes associated with the rice plant, suggest a development within the cultural environment of rice cultivation. In the following list, the entry number in Burrow and Emeneau (1984) for each cognate set is indicated at the respective head:

	#335		#5173		#497		
Kannaḍa	āku	leaf, rice			Kannaḍa	ila	'leaf'
Telugu	ākul	seedling	rĕku	'petal'	Malayalam	ila	'leaf'
Kolami	–		rekka	'leaf'		ilak-ku	'leaflet of palm'
Kuwi	ak(k)u	'leaf'	reku	leaves of rice plant			
Koṇḍa	āku	'leaf'			Tamil	ilai†	'leaf, petal'

† loaned from Malay? -

Set #5173 suggests borrowing from the Austroasiatic protoform for 'rice', and set #335 could be of the same provenance. Set #497 is borrowed from a Munda reflex of Proto-Austroasiatic *slaʔ 'leaf':[8]

'leaf'		*slaʔ		*slaʔ		*slaʔ
Munda and Khasi:	Kharia	*u-laʔ*	Gutob	*o-la*	Khasi	*slak*
Mon-Khmer:	Mon	*slaʔ*	Khmer	*slik*	Bahnar	*hla*
	Darang	*hla*	Samre	*sla*	Khmu	*hlaʔ*
Palaung-Wa:	Palaung	*hla*	Riang	*laʔ*$_2$	Wa	*laʔ*
Viet-Muong:	Khên Muong	*la*$_{C1}$	Vietnamese	*lā*$_{C1}$	< pre-Vietn.	**hlaʔ*
Aslian:	Temiar	*səlāʔ*	Jah-Hut	*hlaʔ*	Mintil	*haliyʔ* -

Gonda (1932: 332–4) suggested that Sanskrit *śalākā* 'chip, splinter, stalk, blade of straw' may be a loan from this Austroasiatic form (see also Kuiper 1948: 79). However, Burrow (1955: 26) has brought forward data implying a Finno-Ugric origin of the Sanskrit word:

Finnish *sale* Hungarian *szilank* 'chip, splinter'

Meanwhile, Mayrhofer (1967: 314–15) thinks both etymologies are unconvincing, and derives Sanskrit *śalākā*, together with *śalka* 'chip, splinter', from Sanskrit *śala* stick, spine (of porcupine)'. Cf. also

Sanskrit *śalya* 'thorn, arrowhead', *śalyaka, śallaka* 'porcupine'.

As indicated by Skeat and Blagden (1906: #L32), a reflex in an Aslian language of the Austroasiatic protoform for 'leaf' was apparently borrowed into Malay as *həlay* 'countword for leaves, pieces of cloth, sheets of paper'. Ronkel (1903: #3) regarded the Malay word as a loan from Tamil *ilai* 'leaf'. Malay is known to frequently drop an initial h-, not however to add a prothetic h-. A borrowing from the Tamil would therefore leave the initial h- in the Malay form unexplained. The Tamil form for 'leaf' derives from the Austroasiatic, but likely Austroasiatic donors exhibiting the required diphthongization of the ultimate vowel are all in the east, particularly in the Malayan Peninsula. It appears, consequently, that unless Tamil *ilai* acquired its final diphthong through suffixation of -i, it must have been borrowed via Malay at an early time, before the grammaticalization of the Malay form as countword. Regardless of whether Sanskrit *śalākā* is a loan from Austroasiatic or not, the Austroasiatic form for 'leaf' did end up as a loan in several Dravidian languages of India, and the Tamil cognate may have been borrowed by a roundabout route over Malay.

SUGARCANE

Rice cultivation is however not the only agricultural activity in which a word for 'leaf, stalk' could have been transmitted from one language to another. The harvesting of sugarcane provides an equally likely economic environment.

Unlike varieties of the wild species *Saccharum spontaneum* L. which occurred over a wide zone stretching from Northeast Africa to the Pacific, the 'noble' cultigen known as sugarcane (*Saccharum officinarum* L.) emerged in a relatively limited geographical area which has been variously located. Jennings and Cock (1977: 51 Table 1) placed it in India and the Far East, Li (1970) in insular Southeast Asia. For a long time, the most widely accepted hypothesis was that of Brandes (1956: 727, 731, 733), according to which *S. officinarum* was ennobled in New Guinea from *S. robustum* Brandes & Jeswiet which grows wild in that region, through human selection. From here it was brought to Northeast India and Southeast China where local natural hybridization with *S. spontaneum* L. resulted in *S. barberi* Jeswiet and *S. sinense* Roxb. respectively. Further studies have suggested, however, that *S. robustus* itself resulted from natural hybridization of *S. spontaneum* with the wild canes *Erianthus arundinaceus* (Retz.) Jeswiet and *Miscanthus sinensis* Anderss. The cultigen *S. officinarum* was selected from populations of the former containing germplasm of all three precursor taxa. The likeliest region for this is the Halmahera and Sulawesi area, where all three are reported to flower simultaneously, providing opportunity for complex hybridization.

Another scenario which fits the genetic data and key chemotaxonomic markers is that the original cultivated sugarcane was not *S. officinarum*, but *S. sinense*, and that this latter was transported by Austronesians into the Archipelago. In this case, the Philippines may be the likeliest place of the development to *S. officinarum* by human selection. *S. sinense* and *S. officinarum* are closely related, and either could have developed from the other, whereas *S. barberi* is quite distinct, and a separate Northeast Indian development (see Daniels and Daniels 1993: 5–6, and literature cited there). The latter of the two hypotheses put forward by Daniels and Daniels, which assumes *S. sinense* from Southeast China as the precursor, seems to agree better with linguistic evidence. Whereas reflexes of the Austronesian protoform for 'coconut', apparently domesticated in insular Southeast Asia, do not occur in Taiwan (see above), reflexes of *təbuS 'sugarcane' (Tsuchida 1975: 151; Zorc 1982: 120; Li 1994: #8) are well represented here, suggesting introduction into the Archipelago from the Southeast China mainland:

'sugarcane'		*təbuS		*təbuS		*təbuS –
Taiwan:	Saisiat	ka-tbuš			Rukai	cubúsə
	Kanakanabu	təvə́sə	Amis	təvuc	Paiwan	tjevus
Philippines:			Tagalog	tubó	Cebuano	tubu
	West Bukidnon	təvu		tubuh-án†		ka-tubh-an†
Indochina–West Indonesia:	Jarai	təbəu	Nias	tovu		
	Sundanese	tiwuʔ	Enggano	kipo-kipo	Malay	təbu
	Old Javanese	təbū	Bintulu	tebau	Ngaju	tɛwu
Sulawesi:	Uma	towu	Buginese	təbbu		

Belau-Marianas:	Belau	dɛb	Chamorro	tupu		
Lesser Sundas:			Sikka	tewwu	Roti	tefu
Central and South Maluku:			Soboyo	tofu	Tifu	tefu
	Yamdena	tɛfu	Kisar	keu	Kai	tɛv
Irian Jaya:			Numfor	kop	Onin	tepi
	Arguni	tof	Waropen	kobow	Minyaifuin	top
Micronesia:			Kusaie	tuh	Trukese	w-ōw
North Papua-New Guinea:			Kaniets	tof	Kaiwa	tov
	Yabem	te	Kove	tou	Nakanai	tobu
South Papua-New Guinea:	Kiriwina	tou	Sinaugoro	tobo		
			Dobu	tou	Motu	tohu
Solomons:			Nggela	tovu	Arosi	ohu
	Vaturanga	tovu	Iapa	tou	Ulawa	ohu
Vanuatu-New Caledonia:	Aulua	tif	Uripiv	top		
	Xaracu Kanakese	de	Mota	tou	Valpei	tobu
Fiji:			Way	tovu	Mbau	ndovu

† 'sugarcane field or plantation'

In India, a 'sugar cane' is already mentioned in the Atharvaveda which dates from before 800 BC (Allchin 1969: 327). Theophrastus reports 'honey produced from reeds' seen in India by Alexander's army (326 BC), but these were probably *Saccharum barberi* Jeswiet which grew in several varieties in the Punjab, Bihar and Assam 'since earliest times' (Purseglove 1972: 217–18). Knowledge of the use of this cane would been acquired by the Indo-Aryans from Austroasiatic autochthones. Indeed, Przyluski (1921: 208–10) and Kuiper (1948: 122) have suggested that Sanskrit *śarkarā* 'sugar' (Prakrit *sakkara*, Pali *sakkharā*) was borrowed from Austroasiatic, listing the following East Austroasiatic forms as evidence:

'sugar'	***sVkar**			***sVkar**
	Khmer	skar	Mon	sakrē ~ thekrē
	Stieng	sɣkar	Chrau	śkor ~ èkor

The etymology is considered with some reserve by Turner (1966: 715), who pointed out that the borrowing must then have been early enough to account for ś- in reflexes in Pakistan. Mayrhofer (1970: 308) mentions the Austroasiatic etymology only sceptically in a footnote, considering the meaning 'sugar' of the Sanskrit word the result of metanymy of Sanskrit *śarkarā* 'grit, gravel, pebbles'. The author relates the latter with Greek *krókē* 'round pebble on

seashore' (plural *krokálai*) and Latin *calx* 'limestone, stone or piece in boardgames', *calculus* 'small stone'.

No Munda cognates have been brought forward for the proposed Mon-Khmer precursors. Furthermore, the Mon-Khmer words do not actually refer to sugarcane, but to sugar, as also the Sanskrit word and its numerous Middle and New Indo-Aryan reflexes (Turner 1966: 715). Whereas Theophrastus writes of a 'honey' from the cane, i.e. a syrupy rather than a gravel or sand-like substance, Dioscorides, writing in the first century AD, reports of 'a honey called *sákkharon* collected from reeds in India and Arabia Felix with the consistency of salt and which could be crunched between the teeth' (Purseglove 1972: 218). The method of refining sugar syrup, obtained from the boiled sap, to a degree that permits crystallization of sugar, was apparently discovered between the fourth century BC and the first century AD in North or Northeast India. This is far too late for a loan from Austroasiatic, considering the first mention of sugarcane is before 800 BC. Therefore, if it was a loan, the borrowing must have been before 800 BC, still having a 'syrupy' meaning, but this is contradicted by the 'gravel or sand-like' meaning of the Mon-Khmer cognates. This circumstance suggests a borrowing around the turn of the Christian era or later, not from Austroasiatic into Indo-Aryan, but the other way round.

YAMS

The original habitats of the greater and lesser yam (*Dioscorea alata* L. and *D. esculenta* L.) were apparently Indochina (Coursey 1967: 9 Fig. 1; see also Alexander and Coursey 1969: 414 Fig. 3). The Austronesian protoform *quBi[] 'yam, particularly *D. alata* L.', has a distribution resembling that of the protoform for 'coconut'. There do not appear to be any reflexes in languages of Taiwan, thus suggesting a southern origin.

'yam'		*quBi[]		*quBi[]		*quBi[]
Philippines:	Aklanon	ubi	Ilokano	ubi	Ifugao	ubi
	Cebuano	ubi	Isneg	ubi	West Bukidnon	uvi
Indochina-Sumatra-Java:§			Jarai	həbəi	Nias	gowi
	Acheh	ubi	Lampung	ubi	Old Javanese	huwi
Kalimantan:	Kelabit	ubih	Sampit	ubi	Ngaju	owi
Sulawesi:§	Banggai	ui	Bantik	ubi	To Bungku	ufi
	Mamuju	upe	Wolio	owi	Salayar	uhi
Maluku and Lesser Sundas:			Ambelau	uwi	Kayali	obi
	Lio	ui	Roti	ufi	Belu	uhi
Irian Jaya:			Wandesi	ubi†	Waropen	uvi-o†

North Papua-New Guinea:			Tolai	up	Nakanai	huvi
	Roinji	gui	Kela	guwi	Mangap	kui
South Papua-New Guinea:			Are	kubi	Kiriwina	kupi
	Boniki	kuvi	Motu	uhe	Bwaidoga	kuvi
Solomons:	Nggela	uvi	Sa'a	uhi	Ulawa	uhi
	Torau	ui	Uruave	uvi	Taiof	uf
Vanuatu:	Lenakel	n-uw	Nĕvhaal	n-ekw	Eromanga	n-up
New Caledonia:			Gomen	kui	Kumak	kuvi-c
Rotuma-Fiji:	Rotuma	ʔuhi	Mbau Fiji	uvi	Nadrau Fiji	uvi
Polynesia:	Samoa	ufi	Hawaii	uhi	Uvea	ʔufi

† possibly recent borrowings
§ with adjacent smaller islands

In Indian literature, yams are first mentioned in the first or second century AD (Allchin 1969: 327). It is unclear, however, whether this refers explicitly to one of the above yam species, because the natural habitat of the (Asian) bitter yam *Dioscorea hispida* Dennst. apparently stretched from India to the Philippines and Maluku, whereas that of the aerial yam *D. bulbifera* L. covers tropical Africa, the Near East, India, Southeast Asia, and the South Pacific (Coursey 1967: 9 and Fig. 1). The relatively late date of the first mention of yams in India suggests an introduction from overseas. However, had introducers of the cultigen been Austronesians, one would have expected to find them among the earlier groups to reach India. By the Christian era, it must be assumed that the principal starch staple of Austronesian newcomers would have been a cereal grain, and not tubers.

Both Dravidian and Indo-Aryan civilization depended upon the cultivation of grain, and considering that a grain cereal, sorghum, was apparently introduced from India to Indonesia at the beginning of regular maritime communication between the two regions, some time between 1000 and 600 BC, Austronesian settlers on the Indian coast probably also took to grain cultivation for their starch staple at an early date. Whatever tubers they also cultivated must therefore have played a subordinate role in their economy. This may have caused a delay in the transmission from Austronesians to Dravidians and Indo-Aryans.

As these latter would at first probably also not accord it particular attention, even more time would pass till a first mention in the literature. But, as long as it is uncertain that the yam mentioned in the literature was indeed *D. alata*, all this remains speculative.

BETEL LEAF AND ARECA NUT

The chewing of betel pepper (*Piper betle* L., Sanskrit *tāmbūla*) in India apparently originates from around the beginning of the Christian era, perhaps from

the last centuries BC. The earliest dated text in which it is referred to by that name is the Mandasor silk weaver's inscription of AD 473 (Gode 1961: 114). Betel chewing is also treated in the Gandhayukti ('Preparation of Perfumes') section of Varāhamihira's Bṛhatsaṁhitā (*c.* 500 AD) (Gode 1961: 158). The same author notes that the Caraka-saṁhitā (first century AD, but there were earlier versions) already mentions *tāmbūla*, without as yet listing lime as one of the accompanying ingredients. Lime is already included among such ingredients in the Suśruta-saṁhitā (last centuries BC, but the surviving version is from the seventh century AD) which also mentions *tāmbūla*. The likely time-interval between the dates of the two medical treatises suggest that the surviving record on betel in the treatise of Caraka is somewhat older than its attested notation in the treatise of Suśruta. The former perhaps dates from the second century BC or shortly afterwards. Chinese *fúliú* 'betel pepper' is first attested in Ji Han's *Nánfāng cǎo-mù zhuàng* ('Herb and Tree Forms of the Southern Parts', fourth century AD, see de Groot 1894: 266–7), and must ultimately derive from Proto-East-Austroasiatic *blu[ʔ] (Mahdi 1994b: 477 n. 155; see also Shorto 1971: 363). In the following list, reflexes in the languages grouped in one column are uniform in either implying presence or absence of the final glottal, or being unspecific in this regard:

'betel pepper'	*bluʔ		*blu[ʔ]		*blu		*blu -
Old Mon	sə-bluʔ	Thèng	blu	Palaung	plu	seventeenth-century Vietnamese	bləw $_{A2}$
Korat Niakuol	a-pluʔ	Rongao	bɣ̆ləu	Riang	plu$_2$		
		Hung	plu	Wa	pu$_2$	Uy-lo Muong	plu $_{A2}$

The form (without glottal) has been borrowed into some Daic languages of Indochina:

Thai phlū $_{A2}$, Shan pū $_{A2}$ 'betel pepper'

It has also been borrowed more than once into Austronesian languages, where a comparison of the reflexes permits the reconstruction of three doublet quasi-protoforms: *bəl[aə]u (Mahdi 1994b: 477 n. 155), *bulu (see Mills 1975: 633) and *buyuq (Blust 1973: #93), cf.

'betel pepper'		*bəl[aə]u		*bəl[aə]u		
Sumatra:	Karo	*bəlo*	Gayo	*blō*		
		*buyuq		*buyuq		*buyuq
Philippines:	Cebuano	*búyuʔ*	Mansaka	*buyuʔ*	Mamanwa	*bozoʔ*
Sulawesi:	Mongondou	*o-buyuʔ*	Buol	*buyu*	Tontoli	*biu*
Kalimantan:	Tarakan	*buyu*	Bulongan	*buyu*	Tinggalan	*buyu*
	Terusan	*dem-buyo*	Tabun	*buyo*	Balait	*buyŏ*
		*bulu		*bulu		*bulu
Sulawesi:			Parigi	*b-olu*	Sausu	*ba-ulu*

	Mandar	*bu-ulu*	Sa'dan	*ba-ulu*	Aralle P.†	*ba-hulu*
Belao-Marianas:			Chamorro	*pu-pulu*	Belau	*ke-búi*
Southeast Maluku:				Wetar	*huru*	
Lesser Sundas:			Bimanese	*bulu*	Belu	*furu-k*
North Papua-New Guinea:			Gedaged	*fu*	Siassi	*ful*

† Pitu-Ulunna-Salu

The distribution of reflexes of ⋆bulu in Melanesia suggests that it was borrowed at a very early date. In the Philippines, West and Central Indonesia it has apparently been replaced by later borrowings from the same source, which tends to confirm the early date of the first borrowing. Mayrhofer (1953–6: 495; see also Turner 1966: 329) has also derived Sanskrit *tāmbūla* from the East Austroasiatic protoform. However, the long penultimate -ū- and the final -a remain unexplained. Even assuming that the Proto-East-Austroasiatic form originally was bisyllabic and had penultimate ⋆u – this appears likely in view of the Chinese and Austronesian borrowings – it must have been short. The ⋆u in final position should have been retained in Sanskrit (e.g. Sanskrit alābū 'bottle gourd', see above, apparently also a borrowing, though not from Austronesian). The word is probably of local, Indic origin. Ramyadeva Bhaṭṭa provided an etymology in his commentary on Cakrapāṇinātha's Bhāvopahāra (Gode 1961: 155 n. 2), but I do not have enough insight into Sanskrit philology to venture an opinion.

Chinese *bīnláng* 'areca or betel nut' is first attested in the Sānfǔ huángtú ('Schematic Plan of the Royal Compound of Sanfu') listing the areca palm as one of the plants grown in the orangery (literally 'lichee nursery') of the royal compound set up in 110 BC (de Groot 1894: 206 n. 1). The contemporary pronunciation was apparently ⋆pi̯ĕnderarchiĕn-lâŋ (see Karlgren 1940: #389a,g–k, and #735a,r,t), suggesting that it was borrowed from an Austronesian language of Indochina or West Malayo-Indonesia (see e.g. Giles 1912: #9247, #6779), for which the protoform ⋆pinaŋ 'areca' can be reconstructed.

'areca'		⋆pinaŋ		⋆pinaŋ		⋆pinaŋ
Indochina:	Cham	*pənɯŋ*	Jarai	*pənaŋ*	Roglai	*pinaŋ*
Sumatra:	Acheh	*pinɣŋ*	Malay	*pinaŋ*	Toba	*pinaŋ ~ piniŋ*
			Karo	*pinaŋ*		
Java:§	Sundanese	*pinaŋ*	Javanese	*pinaŋ*	Madurese	*pɛnaŋ*
Kalimantan:	Ngaju	*pinaŋ*	Maanyan	*pinaŋ*	Tarakan	*pinaŋ*

§ with adjacent smaller islands

The immediate donor may well have been one of the Austronesian languages of Indochina, for which Lee (1966: 209) reconstructed Proto-Chamic ⋆pināŋ. The linguistic data thus seem to suggest that the Chinese acquired betel pepper from Indochina, and the areca nut from Austronesians of West Malayo-Indonesia or

coastal Indochina. But as many Austronesian languages have apparently borrowed the East Austroasiatic form for 'betel pepper' already at a very early date, it is possible that Chinese borrowed both forms from Austronesian languages. In other words, either the two principal vegetable ingredients of the betel-chewing charge, or at least their names, were introduced into China from two different sources, an Austroasiatic and an Austronesian one respectively, or they both originate from an Austronesian source which in turn borrowed the word for 'betel pepper' from Austroasiatic. The latter should probably also be seen as the likeliest way by which the two ingredients and their use reached India.

Indeed, the late date of the introduction of betel chewing in India suggests that a direct acquisition from an Austroasiatic source is unlikely, particularly bearing in mind that remains of areca nuts have been reported in Spirit Cave, Thailand, in layers with radiocarbon dating of 7000 BC (Gorman 1969: 672). The significant position of betel chewing in Austronesian daily life since a very early time is evident from the fact that the word for areca nut in many languages throughout Austronesia reflects the protoform *Buaq 'fruit'. No other vegetable product comes anywhere near such a ubiquitous nomination as the 'fruit *par excellence*'. The only other such products to attain a comparable characterization at least in a limited regional scale, are the locally prevailing cereal staples, e.g. rice or foxtail millet in some languages of Taiwan (see Mahdi 1994b: 437), or the coconut, e.g. in some languages of the Central and Southern Philippines. Many languages have two reflexes of *Buaq, one meaning 'fruit', the other 'areca', of which one sometimes has an affix. In Proto-Oceanic (or pre-Proto-Oceanic), the form for 'areca' acquired a nasal prefix, which, though subsequently fused with the initial bilabial in most languages, is responsible for the appearance of a divergent reflex of that initial (compare with the initial in the respective form for 'fruit' in the Oceanic languages):

*Buaq ~ *(m)Buaq		'fruit'	'areca'		'fruit'	'areca'
Taiwan and Botel Tobago:	Puyuma		*vuáq*	Kanakanabu	*vuáʔəɬ*	
				Yami		*buwaʔ*
Philippines:	Ibanag	*bua*		Ivatan		*buwaʔ*
	Samal	*bua*		Ilokano		*boa*
	Jarai	*bɔh*		Nias	*bua*	
Indochina-Sumatra-Java:	Lampung	*buah*	*buah*	Old Javanese	*wwah*	*wwah*
Kalimantan:	Maanyan	*wuaʔ*		Miri	*b[u]e*	*bue*
Sulawesi:§	Uma	*wuaʔ*		Sangir	*bua*	
	Tontemboan	*wuaʔ*		Banggai	*wo*	*wo*
	To Bungku	*fua*	*fua*	Mamuju	*bua*	*bua*
Belau-Marianas:	Belau	*búuy*		Chamorro	*pugwaʔ*	
Maluku:	Amahai	*hua-ano*	*hua-lo*	Ambelau	*bua-ni*	*bua*

	Nuetetu	*pua-wi*	*pua*	Elpaputih	*hua-i*	*hua*
	Bonfia	*hua-n*	*buå*	Fordata	*vua-n*	
Lesser Sundas:	Sikka	*wua-ŋ*	*wua*	Solor	*fua*	*fua*
	Roti	*boa-k*	*pua*	Belu	*ai fua-n*	*bua*
Irian Jaya:	Iresim	*uwe*		Sarmi	*buʔeh*	
	Onin	*pua-n*		Sobei	*a-fo*	*pue*
Micronesia:				Carolinian	*[bwa*	*ppwu*
	Woleai	*uwa*	*bbuw*	Mokilese	*wah*	*pwu*
North Papua-New Guinea:	Wuvulu	*fua-na*		Papitalai	*mbūa*	*mbūe*
	Maleu	*bua*		Tolai	*bua-i*	
	Yabem	*buʔ*		Mangga Buang	*buak*	
South Papua-New Guinea:	Dobu	*ua*		Kiriwina	*ua*	*bua*
	Sinaugoro	*ɣua-ɣua*	*bua*	Motu	*hua-huabua-tau*	
Solomons:	Fagani	*fua*		Nggela	*vua*	*mbua*
	Sa'a	*hue*	*pue*	Arosi	*hua-n*	*bua*
Vanuatu:	Lenakel	*no-ua*	*na-pwo-k*	Kwamera	*nu-kwa*	*na-pwo-k*
Loyalties:	Iai	*wa-n*		Lifu	*we-n*	
New Caledonia:	Kumak	p^{w}ã-t		Xaracu	p^{w}ã	
Rotuma-Fiji:	Rotuma	*hue*		Mbau Fiji	*vua*	
Polynesia:	Samoa	*fua*		Maori	*hua*	

† 'orange', § with adjacent smaller islands

One must therefore assume that already earliest Austronesian settlers in India and Sri Lanka chewed betel. It probably took a lot of time, before this 'barbaric' or *mleccha* custom was adopted, at first by Dravidians, and finally also by Indo-Aryans. It is nevertheless rather strange that neither the Mahābhārata nor Rāmāyaṇa makes any mention of *tāmbūla*, although the clove, *lavaṅga*, coming all the way from the Moluccas is already mentioned in the latter.

LIME AND LEMON

For the spread of the lime (*Citrus aurantifolia* (Christm.) Swingle) and the lemon (*C. limon* (L.) Burm.f.), and particularly of the words which denote them, we have a complex picture. General consent seems to prevail on origin of the lime in insular Southeast Asia, and the lemon on the mainland and adjacent regions of India (Li 1970: 16; Watson 1983: 44). Earliest mention in India is at a rather later date, i.e. as Sanskrit *nimbū* in the Rājanighaṇṭu (AD 1235–50: see Glidden 1937: 386). However, one may safely assume

earlier knowledge here, because one of the earliest mentions of the lemon in Arabic, i.e. as *līmūnah* in the first half of the tenth century by al-Iṣṭakhri in his Masālik al-Mamālik, ascribes its provenance to al-Manṣūra, a port in the Sindh (Glidden 1937: 382; Watson 1983: 46).

The lime is also mentioned in Arabic for the first time in the tenth century, by Ibn Waḥshīya who called it *al-khashīshā*, noting at the same time that it was known in Persia as *al-līmū* (Watson 1983: 46–7). Beginning from the fourteenth century, Arabic sources differentiate between *līmah* 'lime' and *līmūnah* 'lemon' (also pronounced *laymūnah*), and this pair of names with and without final nasal n before the grammatical ending (-ah) is retained consistently in the chain of borrowings leading from Arabic to English lime and lemon respectively (Glidden 1937: 385). Cognates of both are attested for Persian.

	'lime'	'lemon'		'lime'	'lemon'
Persian	līmū	līmūn	Arabic	līmah	līmūnah ~ laymūnah
Italian	lima	limone			
Spanish	lima	limón	Portuguese	lima	limão
Provençale	lima	limoun	Standard French	lime	limon
English	lime	lemon			

Forms in India and neighbouring countries are mainly without the final nasal. There are two sets of cognates, probably either deriving from early doublets, or representing parallel borrowings from a common source. The one has medial intervocalic -m-, the other -mb-. In both sets, initial l- tends to alternate with n-:

'citrus'	*limu	*nimu	*limbu		*nimbu	
Indo-Aryan:†						
	Baluchi	*līmū*	*nīmū*	Gujarati	*lību*	
	Hindi	*līmū*	*nīmū*	Hindi		*nīmbū, nību*
	Kumaunī	*limū*	*nimū*	Kumaunī		*nību*
	Sindhi	*līmo*		Bengali	*lebu*	*nimbɔgh*
	Sinhalese	*lima*		Panjabi		*nimbū*
Dravidian:‡						
	Kannaḍa	*i-limi-ñci*		Tulu	*limbɛ*	*nimbɛ*
	Telugu	*i-lumi-ccai*		Telugu		*nimma*
	Malayalam	*e-lumi-cca*				

† Glidden (1937: 386–7), Turner (1966: #7247)
‡ Laufer (1934: 147 n. 6), Burrow and Emeneau (1984: #836)

The simultaneous existence of variant forms, with cognates in both sets in one and the same language, and the parallel distribution of both these sets in Indo-Aryan as well as Dravidian, suggests relatively late transmittance by inter-ethnic contact, particularly through the vernacular.

The ultimate etymon of the form with final nasal has not been elicited so far, and has perhaps not survived to our days. It was probably a word in a

language of Indochina or South China. The Chinese cognate *líméng* appears for the first time in a reference to the lemon by Fan Chengda in his *Guì hăi yú héng zhì* (preface dated AD 1157), and shortly afterwards in Zhou Qufei's *Lĭng wài dài dā* of 1178, the latter noting that the fruit was said to have been introduced by 'southern barbarians' (Laufer 1934: 145–6). The term *líméng* was indeed not an item of Chinese vocabulary, but a transcription of a foreign word using Chinese characters meaning 'numerous, black' and 'dim, distinct' respectively (see Giles 1912: #6942 and #7766). The latter character, presently read *méng*, was pronounced *mung* in Middle Chinese (compare Karlgren 1940: #1181a–f, having the same phonetic character). Variant transcriptions are to be found in the various early Chinese sources – e.g. *níngméng* (the characters are Giles 1912: #8332 'k.o. tree with medicinal bark' and #7767 'k.o. tree with yellowish leaves' respectively) and *líméng* (#6942 and #7767), see Laufer (1934: 144–5) – indicating that early writers were often not aware of transcriptions already used by other authors.

Hindi cognates of the form without final nasal were ascribed by Kern (1897: 272–3) to West Malayo-Indonesian reflexes of *limau. The antiquity of the Austronesian protoform is confirmed by the existence of cognates in Oceania, which have however all undergone metathesis (Dempwolff 1938: 97; Milke 1968: #47):

*limau > *limo > *moli.

This feature is shared by all Oceanic cognates, which thus show it to be an exclusively shared innovation indicating an early split-off. In other words, the possibility of a relatively late parallel borrowing (e.g. from Malay-speaking seamen on whalers) can be excluded.

'citrus'		*limau		*limau		*limau
Philippines:			Isneg	*limáw*		
Sumatra:§			Nias	*dima*	Gayo	*limɣ*
			Malay	*limaw*	Lampung	*limaw*
Java-Bali:	Sundanese	*lɛmɔʔ*	Javanese	*limo*	Balinese	*limo*
Kalimantan:			Ngaju	*limaw*	Baluy Kayan	*limaw*
Sulawesi:	Ratahan	*limu*	Gorontalo	*limu*	Mongondou	*limu*
	Bare'e	*lemo*	Balanipa	*lemo*	Makassarese	*lemo*
West Central Maluku:			Soboyo	*limu*	Taliabu	*limu*
Southeast Maluku:			Kai	*lim*	Leti	*lemmu*
North Papua-New Guinea:			Tolai	*muli*	Manam	*moli*
Micronesia:			Woleai	*liu-mwul*†	Carolinian	*lō-mwul*†
Solomons:			Nggela	*moli*	Bugotu	*moli*
			Arosi	*mori*	Sa'a	*moli*

Vanuatu:	Mota	*ŋmol*	Lenakel	*nə-məl-h*	Kwamera	*kwa-n-mər-hi*
Rotuma-Fiji:			Rotuma	*mori*	Mbau Fiji	*moli*
Polynesia:	Futuna	*moli*	Samoa	*moli*	Tonga	*moli*

† relationship uncertain
§ with adjacent smaller islands

Reflexes in the various languages refer alone and in compounds to various Citrus species. The following is an incomplete list for some languages of West and Central Indonesia, with the respective entry number of de Clercq (1909), and with the respective reflex of *limau abbreviated to l.:

No.	Citrus sp.	Malay	Ngaju	Mongondou	English
786	C. aurantifolia Swingle	*l. kəsturi*	*l. nipis*	*l. onta*	lime
787	C. aurantium L.	*l. manis*	*l. palangan*	*l. moromimit*	orange
791	C. decumana Murr.	*l. bəsar*†	*l. guloŋ*	*l. kasumba*	pomelo
792	C. medica L.	*l. kərbaw*	–	–	citron
794	C. ovata Hassk.	*l. suwaŋi*	–	*l. popotuan*	–
796	C. hystrix D.C.	*l. purut*	–	–	porcupine lemon

† also *l. kəsumba*

The Austronesian form must indeed be the etymon of the Hindi forms as assumed by Kern, as also of other South Asian cognates listed above. From here it must have been transmitted with the meaning 'lime' to Arabic, and through the latter into European languages of the Mediterranean and Atlantic coasts. The form without final nasal has also been borrowed into Chinese. Thus the eighteenth-century *Nányuè bǐjì* of Li Diaoyuan points out that *lǐmù* designates the same fruit as *yímǔ* (already attested in a sixteenth-century work) which, he indicates further, was also called *líméng* (Laufer 1934: 148–50). As Laufer noted, the relatively recent term *lǐmù* probably derives from Persian *līmū*.

CONCLUSION

The material on the introduction of actual and alleged Southeast Asian cultigens or names of cultigens to the South Asian subcontinent offers a rather uneven picture. In the case of culture plants of a 'southern' origin, roughly speaking from insular Southeast Asia or South Indochina, reflexes of Austronesian protoform are usually missing in Taiwan (coconut, yams, lime). This also holds for plants from outside, introduced into Austronesia through West Malayo-Indonesia (bottle-gourd). But as in the case of *zawaʔ 'sorghum' (subsequently also 'grain', 'foxtail millet') introduced from India, occasional reflexes may occur in Taiwan as a result of relatively late borrowing (Mahdi 1994b: 432). In the case of a 'northern' origin, i.e. North Indochina and

South China, the distribution area of reflexes include languages of Taiwan (sugarcane, cf. also *pajəi 'rice' in Mahdi 1994b: 434–5).

With regard to relations with India, linguistic data tend to provide a reliable picture, even reflecting nuances which could be appreciated only through the inclusion of the latest archaeological and botanical data. Thus, in the case of sugarcane, a stricter consideration of the linguistic data contradicts an earlier hypothesis of an origin in New Guinea, but fits the more recent hypothesis of two independent origins in North India and the Far East. With the exception of rice, introduced towards the end of the third millennium BC from North Indochina, and the lime, introduced rather late, towards the end of the first millennium AD, the plants or crops from Southeast Asia acquired new names in the process of transmission to India. In these instances, we are fortunate to have historical data on the subject. These permit us to date the introduction of the coconut, areca nut, and betel pepper around the second century BC, although some considerations make it possible that the coconut was introduced some centuries earlier. The second century BC was perhaps also the time of first introduction of the greater yam, but direct evidence is from some centuries later, and there is uncertainty about the species in question.

NOTES

1 With the inadequate tʃ and dʒ replaced by c and j respectively, and IPA j for palatal glide replaced by y. Furthermore, ʃ is replaced by š, and retroflex articulation is generally indicated by a subdot.

2 The principal particularities are that i and y both represent /i/, o is read /u/, whereas ng, dr and tr are digraphs, respectively spelling /ŋ /, and a voiced and unvoiced retroflex affricate somewhat resembling /j/ and /c/.

3 This uses the digraphs dj and tj for the d and t with *cedille* (the 'tail' under French ç) of some other sources.

4 For Vietnamese I use a phonemic transcription, with the toneme indicated as subscript in the notation of Fang-kuei Li, from which that of Chang Kun and Herbert Purnell differs only in that the B and C basic tones are interchanged.

5 Reviewed in Blust (1978). Successive steps in the development of the transcription, departing from that of Dempwolff (1934) by a one-to-one replacement of symbols and a reallocation of reflexes defining *q and *h (the latter presently *S) in Dyen (1947a), are documented in Dyen (1947b, 1951, 1953, 1962, 1965).

6 Zide and Zide (1976: 1304–5); Shafer (1962: 123); Pinnow (1959: #V139); Luce (1965b: #117); Maspero (1912: 22); Skeat and Blagden (1906: #R111).

7 Additional sources: Turner (1966: #12233) for Indo-Aryan; Burrow and Emeneau (1984: #215) for Dravidian.

8 Pinnow (1959: #v50); Luce (1965a: #2); Barker (1966: 16); Benjamin (1976: #57).

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